

# Acta Psychologica

EUROPEAN JOURNAL OF PSYCHOLOGY

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AMSTERDAM



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**B. J. KOUWER (GRONINGEN)**  
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Volume XI — No. 1 (1955)



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## CONTENTS:

PROCEEDINGS OF THE FOURTEENTH INTERNATIONAL CONGRESS  
OF PSYCHOLOGY, MONTREAL, JUNE 1954.



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Volume XI

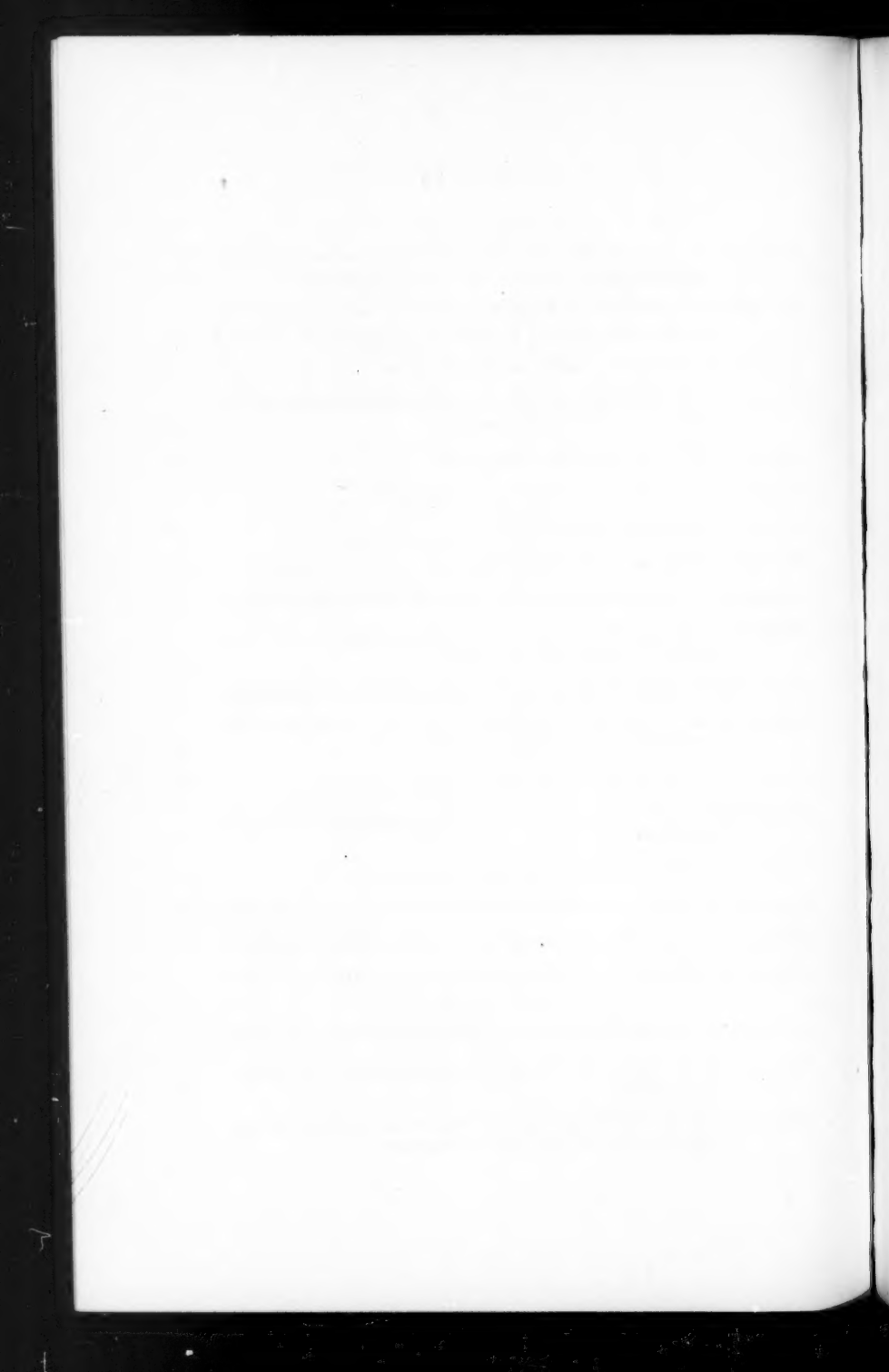
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# CONTENTS

	Page
BIESHEUVEL, S. and D. R. PITT, The relationship between secondary function and some aspects of speed and tempo of behaviour . . . . .	373
BJERSTEDT, A., A double-directed analysis of preference motivations and other pal-description statements: Studies in socio-perceptual selectivity . . . . .	257
CARLSSON, G., Social class, intelligence and the verbal factor . . . . .	269
COHEN, J., C. E. M. HANSEL and J. D. SYLVESTER, Interdependence in judgments of space, time and movement . . . . .	360
DUIJKER, H. C. J., In memoriam Géza Révész . . . . .	357
Editorial notice . . . . .	1
EKMAN, G., Dimensions of emotion . . . . .	279
HERMANN, I., Denken und Sprechen in ihrer gegenseitigen Abhängigkeit . . . . .	513
JOHANSSON, G., I. DUREMAN and H. SÁLDE, Motion perception and personality I . . . . .	289
MEER, H. C. VAN DER, Phänomenale Geschwindigkeitsunterschiede bei Bewegungen in verschiedenen Richtungen . . . . .	475
MUNDY-CASTLE, A. C., The electroencephalogram in relation to temperament . . . . .	397
NYSSSEN, R. and J. BOURDON, Contribution to the study of the size-weight illusion by the method of P. Koseleff . . . . .	467
PIKLER, A. G., The diatonic foundation of hearing . . . . .	432
Proceedings of the fourteenth international congress of psychology, Montreal, June 1954 . . . . .	3
RASMUSSEN, E. TRANEKJER, On perspectoid distances . . . . .	297
RASMUSSEN, E. WULFF, Experimental homosexual behavior in male albino rats . . . . .	303
REUNING, H., A new flicker apparatus for measuring individual differences . . . . .	447
ROMMETVEIT, R., Model construction in psychology: A defense of "surplus meanings" of psychological concepts . . . . .	335
SMEDSLUND, J., The epistemological foundations of behaviorism. A critique . . . . .	412
SMITH, G. and U. KRAGH, Do micro-genetic sequences reflect life history? An illustration . . . . .	504
SMITH, G. J. W. and MAJ HENRIKSSON, The effect on an established percept of a perceptual process beyond awareness . . . . .	346



PROCEEDINGS  
OF THE  
FOURTEENTH INTERNATIONAL CONGRESS  
OF PSYCHOLOGY  
MONTREAL — JUNE 1954

ACTES  
DU  
QUATORZIÈME CONGRÈS INTERNATIONAL  
DE PSYCHOLOGIE  
MONTREAL — JUIN 1954

PROCEEDINGS OF THE  
FOURTEENTH INTERNATIONAL CONGRESS OF PSYCHOLOGY  
ORGANIZED UNDER THE DIRECTION OF  
THE INTERNATIONAL UNION OF SCIENTIFIC  
PSYCHOLOGY  
SPONSORED JOINTLY BY  
THE CANADIAN PSYCHOLOGICAL  
ASSOCIATION  
AND  
THE AMERICAN PSYCHOLOGICAL  
ASSOCIATION  
AND HELD AT  
MCGILL UNIVERSITY AND AT THE UNIVERSITÉ DE  
MONTREAL  
MONTREAL — JUNE 7 TO 12, 1954

ACTES DU  
QUATORZIÈME CONGRÈS INTERNATIONAL DE PSYCHOLOGIE  
ORGANISÉ SOUS LA DIRECTION DE  
L'UNION INTERNATIONALE DE LA PSYCHOLOGIE  
SCIENTIFIQUE  
PLACÉ SOUS L'ÉGIDE DE  
LA SOCIÉTÉ CANADIENNE DE  
PSYCHOLOGIE  
ET DE  
L'AMERICAN PSYCHOLOGICAL  
ASSOCIATION  
ET TENU À  
MCGILL UNIVERSITY ET À L'UNIVERSITÉ DE  
MONTREAL  
MONTREAL, DU 7 AU 12 JUIN 1954

## CONTENTS

	Pages
INTERNATIONAL UNION OF SCIENTIFIC PSYCHOLOGY . . . . .	9
XIV <sup>TH</sup> INTERNATIONAL CONGRESS OF PSYCHOLOGY	
OFFICERS AND COMMITTEES . . . . .	14
REPORT ON THE CONGRESS . . . . .	15
PRESIDENTIAL ADDRESSES	
Edward A. Bott . . . . .	18
Edward C. Tolman . . . . .	31
EVENING ADDRESSES	
Jean Piaget . . . . .	41
Wilder Penfield . . . . .	47
Albert Michotte . . . . .	70
ABSTRACTS OF PAPERS (SYMPOSIA)	
Experimental and theoretical analysis of instinctive behavior . . . . .	93
Projective techniques and psychological theory . . . . .	98
Multivariate methods for electronic computers . . . . .	103
The relation of the person to his environment . . . . .	108
Recent developments in sensory psychology . . . . .	114
Present status of freudian theory . . . . .	122
Individual differences in abilities and their implications for national policy . . . . .	126
Recent advances in conditioning . . . . .	132
Social variables in personality determination . . . . .	142
Mechanisms of motivated behavior . . . . .	147
Some new approaches to the multidimensional analysis of behavior . . . . .	154
Consciousness: revised and revived . . . . .	156
Cerebral functions and behavior . . . . .	162
Social and technological organization in industrial production systems . . . . .	170
Recent trends in perceptual theory . . . . .	176
Motor skills . . . . .	181
Personal contact and change in intergroup attitudes . . . . .	189
European characterology . . . . .	195
Information theory in psychology . . . . .	202
Cross-national research in social psychology . . . . .	206
Problem solving behavior . . . . .	213
Recent developments in color . . . . .	220

LIST OF MEMBERS	Pages
Honorary members . . . . .	226
Members . . . . .	227
Associate members . . . . .	252
Membership . . . . .	254
INDEX OF AUTHORS NAMES . . . . .	255



## TABLE DES MATIÈRES

	Pages
L'UNION INTERNATIONALE DE LA PSYCHOLOGIE SCIENTIFIQUE	9
XIV <sup>E</sup> CONGRÈS INTERNATIONAL DE PSYCHOLOGIE	
OFFICIERS ET COMITÉS . . . . .	14
RAPPORT SUR LE CONGRÈS . . . . .	15
CONFÉRENCES DES PRÉSIDENTS	
Edward A. Bott . . . . .	18
Edward C. Tolman . . . . .	31
CONFÉRENCES DU SOIR	
Jean Piaget . . . . .	41
Wilder Penfield . . . . .	47
Albert Michotte . . . . .	70
RÉSUMÉS DES COMMUNICATIONS (SYMPOSIA)	
Analyse expérimentale et théorique du comportement instinctif	93
Techniques projectives et théorie psychologique . . . . .	98
Méthodes de calculs complexes au ordinateur électronique . . . . .	103
Rapports entre l'individu et son entourage . . . . .	108
Les progrès récents de la psychologie sensorielle . . . . .	114
Etat actuel du freudisme . . . . .	122
Différences individuelles d'habileté et leurs influences sur la politique nationale . . . . .	126
Apports récents dans le conditionnement . . . . .	132
Dimensions sociales de la personnalité . . . . .	142
Mécanismes de motivation dans la conduite . . . . .	147
Quelques nouvelles méthodes d'analyse multidimensionnelle du comportement . . . . .	154
La conscience revalorisée . . . . .	156
Comportement et fonctions cérébrales . . . . .	162
Organisation sociale et technologique de la production in- dustrielle . . . . .	170
Nouvelles tendances de la théorie de la perception . . . . .	176
L'habileté motrice . . . . .	181
Influence des contacts personnels sur les changements d'attitudes « inter-groupes » . . . . .	189
Caractérologie européenne . . . . .	195
Théories d'information en psychologie . . . . .	202
Psychologie sociale sur le plan national . . . . .	206
Le comportement dans les situations problématiques . . . . .	213
Nouvelles découvertes au sujet de la couleur . . . . .	220

LISTE DES MEMBRES	Pages
Membres honoraires . . . . .	226
Membres . . . . .	227
Membres associés . . . . .	252
Inscription . . . . .	254
INDEX DES NOMS DES AUTEURS . . . . .	255

## INTERNATIONAL UNION OF SCIENTIFIC PSYCHOLOGY

### *Extracts from the Minutes of the Executive Committee and the General Assembly of the International Union of Scientific Psychology*

The General Assembly met at the Sheraton-Mount Royal Hotel, Montreal, June 6 at 11 a.m.

Present: H. Piéron (presiding), Sir F. C. Bartlett (Great Britain), H. C. J. Duyker (Holland), J. Elmgren (Sweden), C. M. Fleming (Great Britain), J. Florander (Denmark), P. Fraisse (France), H. S. Langfeld (U.S.A.), N. Mailloux (Canada), D. G. Marquis (U.S.A.), A. Michotte (Belgium), N. Nieto (Montevideo), J. Piaget (Switzerland), M. Ponso (Italy), L. Guttman (Israel), A. H. El Koussy (Egypt).  
Observers: O. Klineberg (U.S.A. representing Unesco), B. Inhelder (Switzerland), C. S. Queiros (Brazil), T. H. Scott (New Zealand), C. Wrigby (New Zealand).

The following important actions were taken:

1. It was announced that the Psychological Societies of Israel and Egypt had been admitted by the previous General Assembly to the Union. It was voted to admit the Yugoslavian Psychological Society to the Union.

2. Several changes in the Statutes were adopted.

3. J. Piaget was elected president and E. T. Rasmussen vice-president of the Union.

4. The following ten members were elected members of the Executive Committee: O. Klineberg, Sir F. C. Bartlett, H. Piéron, N. Mailloux, T. Drever, H. S. Langfeld, A. Michotte, J. Elmgren, H. C. J. Duyker and J. Germain.

5. The motion was carried to change the fiscal year of the Union, July 1st to June 30th, to the calendar year. The present Secretary General and acting Treasurer is to remain in office until December 31, 1954 and the next dues are to start from January 1st, 1955.

6. The budget for administrative purposes of \$2400 was adopted.

The Executive Committee met at the Sheraton-Mount Royal Hotel, Montreal, June 12 at 2 p.m.

Present: J. Piaget (presiding), E. T. Rasmussen, O. Klineberg, Sir F. C. Bartlett, H. Piéron, N. Mailloux, T. Drever, H. S. Langfeld, A. Michotte, J. Elmgren, H. C. J. Duyker and J. Germain.

The following important actions were taken:

1. O. Klineberg was elected Secretary General and J. Germain Vice-Secretary General. N. Mailloux was elected Treasurer.

2. It was voted to leave to the Secretary General and the President the matter of the appointment of an Executive Secretary.

The following societies are now members of the Union: Belgian, British, French, German, Egyptian, Israeli, Japanese, Yugoslavian, Netherlands, Norwegian, Swiss, Swedish, Italian, Danish, Canadian, American, Spanish, Brazilian, Uruguayan, Finnish.

H. S. LANGFELD  
*Executive Secretary*

# STATUTES OF THE INTERNATIONAL UNION OF SCIENTIFIC PSYCHOLOGY

## *Section I. — Nature, Aims, Headquarters and Duration of the Union*

### Article 1

The International Union of Scientific Psychology is a group uniting the National Societies and the Associations of the different countries (see Article 6), having for their aim the development of studies and scientific researches in psychology, whether biological or social, normal or abnormal, pure or applied.

### Article 2

The aims and objects of the Union are as follows:

- a. To contribute to the development of intellectual exchange and scientific relations between psychologists of different countries and in particular for the organization of International Congresses and other meetings whether general or specialized on subjects to be determined.
- b. To contribute to psychological documentation in the different countries by fostering international exchange of publications, of books, and of reviews, of films and of bibliographies.
- c. To aid scholars of different countries to go abroad to universities, laboratories and libraries, etc.
- d. To foster the exchange of students and of young research workers.

### Article 3

The Union may receive subsidies or donations from governments, from inter-governmental organizations, from private or special sources for its general functioning or for the accomplishment of any special tasks which are in accordance with the general aims set out in these statutes.

### Article 4

The central headquarters of the Union will be fixed by a decision of the General Assembly.

### Article 5

No limit will be set to the duration of the Union. Its dissolution can only be decided at a meeting of the General Assembly by a majority of two-thirds of the entire membership of the Assembly, voting by correspondence being permitted.

## *Section II. — Membership of the Union*

### Article 6

The members of the Union shall be national societies of scientific psychology, regularly established, or where there is more than one society in one country, a federation or association which includes all of them.

## Article 7

A country may assist the Union either through its principal academy or through its National Research Council, or through some other national institution or association, or in the absence of these, through its government.

The term "country" is to be understood as including dominions, protectorates and non-autonomous territories in which independent scientific activity has been developed. Federated states will be considered single countries.

## Article 8

Members of the Union may be accepted by a majority vote of the members of the Assembly either at a meeting or by correspondence.

## Article 9

The unit dues shall be ten dollars. A unit consists of 100 dues paying members or less. The number of units assigned to a country shall depend upon the number of psychologists in the societies or associations of that country, but shall not exceed 40 units for any one country. In each country the organization adhering to the Union shall be responsible for the payment of the dues.

## Article 10

The membership of any society or association in the Union shall be terminated:

- a. by resignation subject to a year's notice;
- b. or by a vote of exclusion adopted by not less than two-thirds of the entire membership of the Assembly, after charges have been presented and an opportunity given for a hearing.

A resigning or excluded association forfeits all claim to any funds in the treasury of the Union.

*Section III. — Administration of the Union*

## Article 11

Ultimate authority for the operation of the Union is vested in the General Assembly, which consists of representatives of the member societies, duly attested, each representative having equal power in discussion and voting. Each society is entitled to one representative if its membership is less than 500, and to two representatives if its membership is 500 or more. Vacancies among the representatives occurring between assemblies are to be filled by the society concerned. A society in existence for at least 50 years at the time of the establishment of the Union and whose membership is limited by its statutes is entitled to two representatives. The officers of the Union shall be *ex officio* members of the Assembly.

## Article 12

The duties and powers of the General Assembly include the following:

- a. To elect a President and Vice-President for a term beginning at the close of an International Congress and to extend to the close of the next International Congress. The President and Vice-President shall not be eligible for re-election immediately. The interval between two Congresses should normally be three years unless the Assembly should decide otherwise.
- b. To elect and exclude member societies as provided in Articles 8 and 10.

- c. To adopt a budget to be submitted by the Executive Committee.
- d. To determine the time and place of the following meeting of the Assembly, subject to change by the Executive Committee in case of necessity.
- e. To amend its Statutes by a two-thirds vote of its members, present and voting, or voting by correspondence.
- f. To establish its own rules of procedure.

#### Article 13

Except as otherwise ordered in Articles 5, 10 (b), and 12 (e), questions coming before the Assembly shall be decided by a majority of representatives present and voting, or voting by correspondence, when considered necessary by the President.

#### Article 14

For administering the affairs of the Union the Assembly shall elect by secret ballot an Executive Committee of ten members; five at least shall be chosen from the Assembly. See Article 16. No more than two shall be from any one country. The members of the Executive Committee shall be elected at the meeting of the Assembly which meets at the time of an International Congress and shall serve until their successors are elected.

#### Article 15

The duties and powers of the Executive Committee are the following:

- a. To choose from its membership a General Secretary and Assistant Secretary of the Union, and a Treasurer. The President of the Union is chairman of the Executive Committee.
- b. To authorize expenditures in conformity with the budget adopted by the General Assembly.
- c. To present the agenda of business to be transacted at the regular meeting of the Assembly.
- d. To arrange for congresses and colloquia.
- e. To call a special meeting of the Assembly in an emergency and to alter the time and place of a regular meeting as provided in Article 12 (d).
- f. To present a report to the next meeting of the Assembly.
- g. To appoint special committees for the discussion and recommendations concerning any questions falling within the purview of the Union.
- h. In general, subject to the superior authority of the Assembly, to make all decisions necessary for the operation of the Union and the realization of its aims, in the intervals between meetings of the Assembly.

#### Article 16

The president of the Union shall preside at all meetings of the General Assembly. He and the Vice-President shall be *ex officio* members of the Executive Committee, which means that this Committee will consist of a total membership of twelve.

#### Article 17

The duties and powers of the General Secretary are the following:

- a. To authorize particular expenditures in accordance with general instructions from the Executive Committee.

- b. With the approval of the President of the Union, to call meetings of the Executive Committee or to arrange for a vote by correspondence of its members, whenever a decision of the Committee is necessary or desirable.
- c. To prepare the minutes of the meetings of the Executive Committee and of the Assembly and to arrange for records of the scientific congresses and colloquia of the Union.
- d. In general, to perform the customary duties of his office and to represent the Union as required by the Executive Committee.

#### Article 18

The duties and powers of the Treasurer are the following:

- a. To present a budget for adoption by the Executive Committee and the General Assembly.
- b. To bank the fees, subsidies or donations.
- c. To authorize payments on the decisions of the General Secretary conformable to the items of the budget.
- d. To present his accounts annually to the Executive Committee for approval and to the General Assemblies at their regular meetings.
- e. To send out due bills annually to the adhering organizations.

XIV<sup>TH</sup> INTERNATIONAL CONGRESS OF PSYCHOLOGY  
XIV<sup>E</sup> CONGRÈS INTERNATIONAL DE PSYCHOLOGIE

*Co-Presidents — Présidents conjoints*

EDWARD A. BOTT  
EDWARD C. TOLMAN

*Organizing Committee for the Congress*

*Comité d'organisation du Congrès*

D. G. MARQUIS, *Chairman/Président*  
N. MAILLOUX, *Secretary/Secrétaire*  
G. A. FERGUSON, *Treasurer/Trésorier*

G. BARBEAU — E. A. BOTT — D. O. HEBB — G. W. KISKER — H. S. LANGFELD —  
R. B. MACLEOD — N. W. MORTON — E. B. NEWMAN — C. C. PRATT —  
R. W. RUSSELL — F. H. SANFORD — D. C. WILLIAMS.

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*R. B. MACLEOD, Chairman/Président*

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— C. COOMBS — H. C. J. DUYKER — J. J. GIBSON — C. H. GRAHAM — E. R. HIL-  
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QUIS — A. W. MELTON — D. A. R. MOFFATT — C. T. MORGAN — G. MURPHY —  
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A. T. M. WILSON — D. WOLFLE — C. F. WRIGLEY.

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N. MAILLOUX, *Secretary/Secrétaire*  
G. A. FERGUSON, *Treasurer/Trésorier*

G. BARBEAU — D. BÉLANGER — D. BINDRA — J. W. BRIDGES — THÉRÈSE DÉCARIE  
— R. LUSSIER — LOUISE MALMO — R. B. MALMO — A. PINARD — W. N. MCBAIN  
W. R. THOMPSON — E. C. WEBSTER.



## REPORT ON THE XIV<sup>TH</sup> INTERNATIONAL CONGRESS OF PSYCHOLOGY

At the XIIIth International Congress of Psychology held in Stockholm in 1951, the newly-formed International Union of Scientific Psychology accepted the invitation of the Canadian Psychological Association to hold the XIVth Congress in Canada with the American Psychological Association as joint sponsor.

An interim committee of the Union, appointed in March 1952, constituted an Executive Committee for the Organization of the Congress in June of that year. The committee was composed of six persons nominated by each of the American and Canadian Psychological Associations. The secretaries of the two societies as well as the secretary-general of the Union became members of the committee ex-officio.

This committee met for the first time at Cambridge, Mass., on January 24th, 1953. The following officers were elected: Chairman: Donald G. Marquis; Secretary, Father Noël Mailloux, O.P.; Treasurer, George A. Ferguson; Chairman Program Committee, Robert B. MacLeod; Chairman Local Arrangements Committee, Donald O. Hebb; Chairman Committee on Placement, E. B. Newman; Chairman Procurement Committee, N. W. Morton. Meetings of the committee were held in April and September of 1953 and in January, April and June of 1954.

The Executive Committee set up four sub-committees (1) to prepare the program, (2) to procure travel funds and find lectureships for overseas psychologists, (3) to find and select persons for the available lectureships, and (4) to make local arrangements in Montreal.

### MEMBERSHIP

One of the first concerns of the committee was to distribute announcements of the Congress and to invite applications for membership. Announcements appeared in various professional journals and were sent to the secretaries of psychological associations affiliated to the Union, who had offered to distribute them to their members. Announcements were also sent to departments of psychology at universities in countries where there are no national associations. All countries were invited through their legations in Ottawa, to send delegates to the Congress. Canadian legations abroad were advised that the Congress would take place and were asked to assist delegates when possible. Membership fees were set at \$15 for full members from North America and \$5 for members from other parts of the world, student members and associate members.

Registration for the Congress was rather slow, reaching a small peak about the May 1st deadline for receipt of applications. Another small peak was reached in the week before the Congress. Three special notices were mailed to members of the American Psychological Association in March, April and May 1954. Special notices were also sent to members of the Canadian Psychological Association and the Eastern Psychological Association in the U.S.A. Just before the Congress, registration

was 727 members and 119 associates. Late registration increased these figures to 876 members and 144 associate members. 31 Countries were represented at the Congress.

#### AID TO OVERSEAS PSYCHOLOGISTS

The Executive Committee made every effort to raise funds to provide travel grants to enable overseas psychologists to attend the Congress. Grants for overseas aid were received from the National Science Foundation in Washington, Mr. Walter M. Stewart of Montreal, Mr. Louis Roberts of New York, the W. B. Saunders Company, Philadelphia, and the Maritime and Ontario Psychological Associations. Money was also appropriated from the general Congress budget. Travel grants were given to 59 persons from 19 countries, larger grants being given to participants in the Congress program. A total of \$18,370 was distributed. It was also possible to arrange for several summer appointments and lectureships to provide small stipends to overseas psychologists. This often enabled them to obtain grants from their own universities and governments.

#### PROGRAM

The Program of the XIVth Congress represented a departure from those of previous congresses. There was no general solicitation of papers, however, suggestions of session topics were welcomed.

The opening session of the Congress was held on Monday, June 7th at 3 o'clock in the Salle des Promotions of the Université de Montréal. The delegates were welcomed by the Honorable Brooke Claxton, Minister of National Defence, as representative of the Government of Canada, by Monsieur Daniel Johnson as representative of the Government of the Province of Quebec, and by Dr. Léon Lortie representing the Mayor of Montreal. Principal F. Cyril James and Vice-Rector Monsignor Georges Deniger spoke for McGill University and the Université de Montréal, while Professor O. Hobart Mowrer and Father Noël Mailloux, O.P., welcomed members on behalf of the American Psychological Association and Canadian Psychological Associations. Professor Henri Piéron gave the presidential address as President of the International Union of Scientific Psychology.

Plenary sessions were held in the evenings when members heard five evening addresses. During the day, from Tuesday to Friday, symposium and communications sessions were held in three simultaneous sections. Also scheduled were film sessions, and scientific and commercial exhibits. Congress sessions ended on Friday afternoon, however, members were invited to participate in an open session of the Unesco symposium on "The Evaluation of International Action Programs", which was held on Saturday morning.

#### COMMUNICATIONS

Congress members met informally at a reception given by the City of Montreal at the Chalet on Mount Royal. There were two lounges for their use and light lunches were served on the lawn opposite Congress headquarters. Members could send and receive messages at the Congress message bureau. This system, however, has limitations and the use of a message repeater at various times during the sessions would have made the bureau more efficient. No list of members was published and members were asked to consult a visible file directory.

## FINANCE

Funds for the organization of the Congress were received from several sources. The American and Canadian Psychological Associations provided loans of \$10,000 and \$750 respectively. \$11,765 was collected in membership fees, while the net income from commercial exhibits was \$1,180.

## LADIES PROGRAM

A program for the wives of members was arranged by the Ladies Committee of the Congress. It included visits to places of interest in Montreal, courtesy luncheons at two large department stores, a tea and an excursion and luncheon in the Laurentian mountains near Montreal.

PRESIDENTIAL ADDRESSES — CONFÉRENCES DES  
PRÉSIDENTS

MONDAY JUNE 7 — LUNDI LE 7 JUIN

*Chairman — Président*

HENRI PIÉRON

*Professeur honoraire au Collège de France  
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INFLUENCES OF ORGANIZATION ON PSYCHOLOGY AS  
A SCIENCE

BY

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*(Professor Bott was introduced by James Drever,  
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I am grateful for the kind and too generous words of introduction by Professor Drever, and also I am greatly honoured, sir, in being nominated by our Canadian Psychological Association as a co-president for this 14th International Congress of Psychology. This is a notable occasion for us in Canada. It is our first opportunity of welcoming the Congress to our country and in light of our relatively recent appearance on the stage of psychological science and of the very modest contributions we have made as yet to psychology, my fellow Canadians and I are frankly pleased and even proud to have the Congress here on our side of the invisible national line on this continent.

For this meeting, however, we in Canada are not your only hosts. In fact the occasion is a joint party. Your senior host, I may say, is the American Psychological Association with Professor Tolman as co-president for this meeting. But I hasten to assure you that this dual plan of officers was not designed as a kind of psychological experiment in order to try out

your endurance as regards presidential addresses. On the contrary it is desired rather to extend you a double welcome.

But while on this point of plural presidents, speaking for myself, I wonder if you do not share my curiosity about why the junior co-president should be asked to lead off at this first of our main sessions, following our formal opening ceremonies of this afternoon. On this question I can only say, sir, that any convincing answer completely eludes me. It may be due to some mystery of protocol, or more likely to some error in judgment on the part of your programme committee, or perhaps only the alphabet is responsible for putting B before T. Whatever the reason, may I remind you that at every good feast, whether for the body or the mind, the choicest portion is always offered last. So, shortly, your senior host, co-president Tolman, will offer you something more tasty and substantial than I shall.

For my part as Canadian co-president I propose to touch briefly, not on any technical topic in psychology, but on some points that have to do with community organization and relationships which seem to affect most of the situations where psychologists nowadays carry on as psychologists. Broadly speaking, what is the rightful place, if any, in our society for psychology and psychologists? Should the psychologist's role be only that of a scientist, or also of a practitioner? And if he is to enjoy both of these roles at once, what conditions will best enable him to do so?

By way of approach to this subject may we first look back historically to gain perspective. Only two or three decades ago, many of us will have heard or even shared in heated debates about whether psychology was in fact a science, or whether it was still mainly philosophical and speculative. Happily those questions no longer excite psychologists or anyone else. Today we psychologists no doubt remain well aware of many shortcomings and limitations in the methods and the precision of our work, due largely perhaps to inherent difficulties in our chosen task of understanding the complex activities of persons. Nevertheless, we have no hesitation and indeed some confidence when it comes to formulating our problems for study, designing experimental means for objectivity, attacking our problems, interpreting our qualitative of quantitative results for purpose of generalization and enunciation of principles. And even if such procedures fail to plumb all the depths of man's nature, at least, we have served our apprenticeship as empirical scientists and we can now claim to belong in that brotherhood, although we still have much to learn about our trade as scientists. It may thus be said that in the generation just past, doubts about psychology as a science have largely been settled; but now in the generation ahead we face questions about psychology as a profession.

What circumstances have led us into this form of development or predicament, whichever one chooses to call it?

Those of us who have witnessed the steady progress of our science since the turn of this century will be cognizant also of certain other concurrent changes in the scope and trends of psychology. Its emergence as a science no longer means that psychology remains restricted to factual inquiries that are conducted with human subjects or animals under controlled conditions in a laboratory, or perhaps within the facilities of a special clinic, or in some other expertly controlled setting. Psychologically trained personnel now operate beyond these more rigorously circumscribed situations, namely, in numerous community settings which may conveniently be termed areas of application. In such areas human subjects are dealt with but under more ordinary conditions of control such as characterize every-day living. Yet, however wide and varied current undertakings by psychologists in the field may be, the sanctions and conventions that govern human contacts in those situations are not less important than they are in a laboratory. The fact is that modern psychology viewed in perspective shows, first, its emergence as a cloistered scientific procedure and then in addition its emergence into wider settings where its undertakings clearly are expected to have some practical significance. In this latter respect at least, psychology, thus extended, will presumably be some form of practice whether there was any intention or any step taken in organization to have it so. This growing tendency for psychology to pass beyond conventional laboratory studies toward various applications that have practical import is the main point we wish to consider. Are any essentials in organization pertinent for areas of application which would serve to advance the interests of our science and its practice?

Although this tendency toward applying our science, usually, has been a gradual process and limited as to the areas concerned, its occurrence may on occasion be abrupt and impressive. To illustrate this latter sort of transition in interest, may one be allowed to inject an instance drawn from personal experience? I well recall the circumstances which first compelled me to recognize how close the relation can be between psychology as an experimental science and as a type of practice. In 1916, while working as a junior on our university staff, I was engaged on a laboratory study of analyzing the experiential and muscular-motor components involved in voluntary movement, using simple reciprocation of the index finger as the problem for study, with graphic recordings of the variables concerned in that action. At that time it happened that severely wounded men from

our overseas force were first being returned from Europe, many with muscle-nerve disabilities from peripheral or central injury, also amputation cases, tendon transplantations, and other conditions of marked motor disfunction. In spite of expert nerve and muscle surgery which repaired the necessary organic connections, the expected return of voluntary function in many of these hospitalized patients was slow or notably slight.

I had, however, recently seen the work of Franz and Lashley in the former's psychological laboratory at Washington<sup>1</sup>, where activity treatment had proved helpful for speeding the recovery of hemiplegic paralysis they had induced in monkeys through experimental permanent injury of a motor cortical region. With their suggestive experiments in mind, some trials by similar active measures were immediately begun in our university laboratory with a few selected veteran cases who had long-standing limb paralysis following peripheral nerve severance and subsequent suture. It was soon found with the majority of these cases that special motivation, coupled with systematic practice on suitably graded exercises, brought progressive improvement in muscle function. An official request was shortly received to elaborate these principles in a near-by military building, to be used mainly for this purpose. Within a few months an effective programme was devised. It emphasized patient participation, aiming to capture the interest of the veteran in helping himself. Beginning with detailed measurement of the motor disability, it used objective means of motivation to direct his attention on what movement he could achieve with the affected part, no matter how small or weak or tardy such movement at first might be. For initial stages, when the mobility was minimal, simple equipment proved essential merely to indicate plainly to the patient through sensory observation, by visual and auditory means as well as by kinaesthetic experience, the actual motor achievement and improvement, if any, from his efforts. Graded exercises were thus separately planned for all the handicapped mobile segments of the body. Each patient daily practised his deficient movements under supervision for limited periods. A patient's own learning curves were likewise used to stimulate his progressive effort and level of aspiration. Finally the target would become the co-ordination of these improved simple movements in order to facilitate normal manipulation and locomotion so far as the organic condition might permit.

What part did organization play in this application? Several colleagues from psychology and other disciplines, too, co-operated in this local

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<sup>1</sup> Franz and Lashley—Recovery from experimentally induced hemiplegia—*Psychobiology*, 1, 1917, 1.

remedial undertaking for veterans, and this practical programme was presently reinforced by a research programme conducted on animals (rabbits) through co-operation of several scientific departments in Arts and Medicine. Then a corps of women assistants was recruited through the army medical branch to be trained at this local centre to work elsewhere on muscle-function re-education. They were later posted to numerous remedial centres across the country. The military and medical authorities in charge of this particular application facilitated the various steps in its development and organization without hesitation. From this start with veterans our present university courses in physical therapy and occupational therapy emerged and have continued since 1920 for dealing with comparable cases that arise in civilian life. But in citing this lengthy description of a particular application, one should also add that from our psychological point of view the provocative problems we encountered in this practical programme of re-education proved more stimulating and enlightening scientifically than any laboratory experiments we ever contrived for studying voluntary movement with the intact organism.

Many of you as scientists may have had similar experiences or have had other contacts with fields of practice. In any event, how do we psychologists today regard various practical demands which confront our growing science in terms of community settings where psychological principles could apply? Should we ignore such challenges or embrace them, and with what safeguards? Or are we too busy with teaching and research to give careful thought to problems beyond our laboratory? Can we assume that such demands represent only a passing circumstance, some temporary emergency? Or, is this trend toward applying science in daily affairs becoming more and more a common-place of our way of life? I incline to the latter opinion but as regards psychology I also ask how its present trend in that direction can best be guided or implemented. Should psychologists be ready to contribute when they can, as indeed they often have done? The development of Child Study and Parent Education in several universities across this continent and in many communities, rural and urban, within the past three decades, is another case in point which derived directly out of scientific psychology when the latter, about 1911, discovered the young normal child as a suitable subject for study. Nor may the relation between science and its application be peculiar to psychology. Do we not find an earlier precedent through noting the path along which natural science itself evolved? The latter, as everyone knows, emerged first from natural philosophy, then took shape as specialized laboratory disciplines and in due time gave rise to the establishment of



Applied Science and Engineering. Such comparison may obviously be hazardous, particularly when we turn from the natural sciences to the social sciences and then to psychology. For it must be admitted that our science may even be ambiguous in its position (at least in the eyes of some university administrations) regarding whether to rank it among the social or the biological sciences. But taking our science as we have it, for better or for worse, is there not this unquestionable trend in our day toward the acceptance and hence the implementing of several applied settings which we as scientists can hardly ignore and may properly foster? In the past, however, psychologists have differed widely on this issue.

Some of our earlier distinguished psychologists, for example, Titchener, held in no uncertain terms that any thought of considering psychology useful should be kept wholly distinct from its programme as a science. On this basis he contended there could and should be a pure science of psychology, limited, of course, as to content and method, but free from all contamination of utility or application. And at the opposite extreme in psychological opinion, arose in America a behaviouristic view, originally popularized perhaps crudely in terms of stimulus and response, which would regard psychology as mainly if not wholly a science for practical use, applicable genetically through the whole span of life. On this basis, one recalls, for example, Watson's claim that beginning with a normal healthy infant, the human being could be moulded by training to be proficient in any skilled profession that might be desired. Between these two extreme positions, other views have seen in psychology a way of studying man which would be both a form of science and a form of practice, with priority to be given in various degrees either on the side of environmental determination or of self-determination.

Without taking sides among these schools of psychology or particular philosophies of education that relate to them, what verdict do the facts seem to warrant on the moot question about our science and its applications, taking into account past trends in psychology, its current interests as revealed in the programme of this Congress, as well as our varied experience as psychologists during two world wars? From this background, and granting broadly that the subject-matter of psychology deals with activities of persons, a first point in my thesis is that some appreciation of value, or utility, is bound to be a relevant factor in all psychological inquiry. If this be accepted, let us at least be cautious about presuming to distinguish between our science as such and its applications by trained personnel. The kind and prominence of the utility feature just mentioned may vary widely, of course, with the nature of one's problems, but is it

ever wholly lacking? Thus, even if our search be merely to ascertain dependable knowledge in the form of observable facts as revealed through repeatable experiment, utility in a particular guise seems to be involved, just as it more obviously is involved should our aim frankly be to restore mental health or to reduce criminal conduct. And likewise, when one turns from remedial or corrective interests, to the pursuit of positive tasks, utility similarly remains no less evident, when for instance we study problems in how to classify, train and utilize personnel for the armed services; or to achieve maximum productivity; or to master some prescribed curriculum in school, or other desired goal. More is at stake, I feel, than a mere exercise in semantics, in stating that all such settings involve utility in the sense of being human activities pursued to attain some end and that they all deserve psychological study. Such a statement, however, means that judgments about the simplicity, the purity, or even the worthiness of the end in view, is not what makes our quests scientific but whether our inquiries add to the store of known facts and general principles concerning them. In sum, when observing or experimenting with persons, I suggest that any claim to distinguish meaningfully between a pure and an applied procedure loses point; in the last analysis, no such line can be drawn systematically for psychology. Today the old dichotomy between science and its application is not good enough. Our scientist is responsible for seeing that applications are scientifically conducted, are in the hands of trained personnel, and that the same ingenuity which formulates questions for research in a laboratory shall also operate for applied settings in the field. How can we promote these goals?

If this point of departure be sound, our question will concern two major issues. First, the production of psychologists; second, their effective integration into the organized life of our day in order that they may apply their special training for their own satisfaction and in the service of their fellows. Here only brief reference can be made in turn to these issues and their relevance with respect to social organization and structure.

In this age of specialization we psychologists have an important part to play on our sector of the broad front that is concerned with the producing of trained personnel of many specialized kinds. In particular, the production of psychologists is initially a responsibility we carry within university communities. In that setting what considerations should control the quality and the number of psychologists to be produced? Or, is control of any sort deemed to be desirable or feasible at that training level? Let us glance at how this production process has evolved and is now operating at least in some settings that are most familiar to us and without venturing

to generalize on patterns pertaining to other places you represent.

Most of our pioneer leaders in psychology were wholly pre-occupied with teaching and research in a few of the larger universities. There the teaching of psychology presently expanded in two main directions: First, a programme in the non-professional Faculty of Arts for undergraduates who elected some degree of concentration in psychology and often proceeded to postgraduate study. Second, a more limited amount of instruction in psychology for students in many of the professional faculties whose fields concerned contacts with people and hence might involve psychological principles: Medicine, Engineering, Law, Pedagogy, Theology, Nursing, Child Study, Household Science, Social Work, etc., are obvious instances. With few exceptions students training for professional careers in these latter specialties would not be psychologists in any strict sense of the term though they wished to be knowledgeable about modern psychology and its significance for their respective fields. This contribution of psychology to other specialties, though secondary for our science, has enlarged over the years and will no doubt continue.

The main direction, however, of university effort for the production of psychologists, continues to be through the undergraduate Faculty of Arts, followed by advanced instruction and research for post-graduate experience and degrees such as Master of Arts and Doctor of Philosophy. These academic degrees, of course, do not in themselves provide professional status, and they often provide little or no practical experience in organized fields of application. On the other hand, graduates with these or analogous degrees constitute by far our main output of well trained psychologists. The problem is how best can they proceed toward a career which will utilize their special training and best serve their community. This is a complex issue as regards human relations and effective organization. Actually it concerns us who teach psychology, our students who aspire to qualify for psychological employment, our scientific associations of psychologists, various employers of psychologists and the clientele they respectively deal with, as well as governmental bodies concerned with public service. Both men and women students are involved, and for the latter this problem of community orientation toward a career in fields of application may be further complicated through the personal decision for the majority of women with respect to marriage and home-making.

The production and placement of psychologists on this continent was not particularly an issue during the first decade or two of this century because at that stage any career openings for graduates who had specialized in psychology and wished to pursue it, were mainly limited to

academic posts in other colleges. This situation has gradually changed as the supply of trained psychologists increased and demands for their skill arose in various quarters. The present occasion is not an appropriate time, of course, to discuss in detail the many factors within particular communities and training programmes which would be pertinent if a reasonable balance is now to be aimed at between the supply and demand for qualified psychologists. As may be expected in the emergence of a potential profession, questions of general qualification, particular specialization, status, future prospects, salary rates, discipline, etc., all arise and will require time and patience to solve. Moreover, ways and means for coping with these issues are already being carefully considered by our scientific associations.

But, in what way do applications of psychology also depend on organization within the community to be served? On this point, surprisingly, there seems to be a conspicuous lack of discussion, if not indeed of interest on the part of psychologists. Or is the proper answer so plain and obvious as not to require mention? Universities produce psychologists only in respect to a limited kind and amount of basic training. Moreover, the university programme in Arts, designed to provide, we say, a liberal education, does not profess to qualify undergraduates technically for any particular kind of practical service, (notwithstanding the expectation by many of our undergraduates that it should do this, as do university programmes in the professional faculties). And even at the postgraduate level, when universities aim to advance our science, and perhaps establish a unit or centre for Applied Psychology in order to train suitable persons for one area or another, do these training facilities also prepare places for them? Again, as regards experience in research, while the university setting affords initial opportunities, this is no longer by any means the only setting where psychological research can be done. There was a time when it was generally thought that to promote good research required only to find a man with an idea, provide him with material facilities and leave him undisturbed. Today psychological research outside a university laboratory usually requires that the field setting be structured in a way calculated to help promote such inquiry. In short, the university's job is not to prepare the field for psychologists but to prepare men for the field.

Again, can a scientific association of psychologists assume any responsibility in effectively organizing local settings as areas of application. Useful though an incorporated scientific society may be in respect to its individual members, such a body is no more responsible than is a university for actually creating or filling posts for psychologists. It may, of course,

assist in essential steps looking toward the certification of individuals by differentiating and listing trained psychologists in terms of their competence for particular types of employment. But neither such a scientific body nor a certifying board will organize the community settings in which psychological work may be required.

Notwithstanding these circumstances, our developing science through the personnel we train is today reaching into the community, and as viewed from the outside a growing need is felt for such assistance. Speaking in the broadest terms, existing areas of application can be classified as pointing in either of two directions, which differ on the basis of how the situation is socially structured. On the one hand are settings in which a special place has been recognized and a post created for the employment of one or more psychologists who have appropriate qualifications, with perhaps similar provision, too, for other types of scientists. The existence of such positions, rather than the procedures by which they come to be established, is a main point about community organization that here concerns us, because the presence or absence of such organized posts significantly influences the applications of our science, and will do so increasingly as time goes on. And this is not to say that problems in organization will be over even when particular posts for psychologists exist, but the problems then would be different and not insurmountable. In the case of many areas which command public interest such positions for psychologists already are established or are in a formative stage. Frequently they are administered under appropriate branches of government at a federal, provincial or municipal level, for example, in such areas as Defence, Health and Welfare, Law enforcement, Education, Labour interests or the Civil Service, etc. In other instances the organized posts may not be governmental in character but have place instead within some existing social structure such as a private industry or philanthropic agency.

In the opposite direction, are those settings where psychologists individually or as a group of private associates undertake psychological service on their own initiative upon a fee basis, with or without sanction by license or other means, offering such service as they may choose or may consider to be in demand. Occasionally a psychologist, for economic or other reasons, may endeavour to function in both the above directions, namely, by holding an established post and also operating to some extent as a person self-employed or in business as a private individual. Our main point, however, is that the self-employment basis as such possesses little or no permanence in the organization of the community that is served. In effect the person who is self-employed and the place he occupies in this

capacity are in fact indistinguishable. Thus, with an unstructured setting, if a practising psychologist ceases to operate, his practice disappears, too, for the community. In other words, the position is in no sense an entity that can be identified as such and that may be filled when vacated.

Points for or against advocating a structured type of setting rather than the unstructured type with respect to psychology for the future may no doubt remain a debatable or even contentious topic that will provoke discussion for some time to come, particularly in the early stages of a science that is still feeling its way into several areas of application. I would say, however, that the relative merits as between settings formally organized and those privately initiated may best be considered from the outset in terms of general principles that are involved and are likely in the long run to affect the interests of psychologists and others concerned. Four points of principle in this connection may be mentioned in conclusion.

First, continuity of contacts. It is well known in psychological studies that not only must numerous cases be studied in order to take account of individual differences, but also that successive contacts with the same individual longitudinally pursued over a considerable period of time are important in order to evaluate one's original impressions in the light of later cumulative findings. This sequential process was soon found to be indispensable, for example, in psychological work with the armed services. Decisions made on recruits at the stage of initial selection required follow-up of the actual outcome at successive training stages and still later, so far as possible, during service experience. Similarly, in studies on children at the pre-school stage and all later levels, continuity of contact is desirable in order more surely to understand the pliable individual we study. Illustrations could be indefinitely multiplied, in industry, in clinics and many other settings. Continuity of contact for psychological work is most readily maintained if there are established posts, designed to facilitate and perpetuate it.

Second, differentiation of complex functions. Unless essential variables can be separately and longitudinally studied, the advantages of experiment combined with critical analysis are likely to be overlooked, and vague impressions to be accepted instead. This became very apparent, for instance, in wartime training of aircrews. Three distinguishable functions were regularly required of the staff who dealt with individual recruits at every aircrew training school: namely, first, instruction in flying, second, final assessment of performance by all cadets, and third, decision about whom to pass or eliminate when the course concluded in a given time. So long as the instructing, the assessing and the eliminating had to be done

by the same training staff, these three related functions tended to interfere mutually to an extent that militated against the effectiveness of the training process as a whole. Psychologists connected with the service who were in a position to study the training programme in detail both as a total sequence and in terms of its constituent functions were able to demonstrate that separate expert staffs for instructing, for assessing results with trainees and for deciding on disposal of trainees, would be measurably more effective both for the service and for the individual recruits. The solution for this sort of differentiation meant a change in the service organization. But our main point is that even the existence of this problem had only been revealed through the way in which a staff for psychological research was set up within the service structure. This war-time innovation of having a special place created for the psychologist as a scientist to study air-training has continued as a permanent feature of the service organization in which it was then tried. A private or volunteer psychologist would have little chance of being as effective for that setting.

Third, to contribute in a practical setting a scientific staff can best operate in an advisory rather than an executive capacity. To marshal all the facts relevant to possible alternative plans of action is a different function from having to decide and instigate a particular course of action based on the facts. Here, again, these functions in the hands of one and the same person can easily become so intermingled that neither is performed with thoroughness and clarity. Choosing courses of action is particularly liable to raise ethical considerations and lead to pre-occupation on codes for conduct and means for instituting or enforcing them. Briefly, when complex areas of application are at issue, the several functions that these involve usually require separate attention and perhaps special organization for fact-finding as distinct from policy making or implementing. This analytical approach will be greatly facilitated if the scientist who objectively makes such analyses has no responsibility also for taking executive action on such findings. In contrast, a psychologist working on a self-employment basis must deal as best he can with all angles that arise in the setting where he works.

Lastly, the factor of economic security. A well trained scientist is entitled to an assured and reasonable living for his services. When he represents a discipline which is by no means yet formally established as a profession the difference between occupying a recognized post at a given salary or engaging in practice individually on a private fee basis will be obvious in principle. If he is to enjoy freedom to work at his best, to plan and engage on some research, to publish in the technical literature the



results of his labours and hence to continue as a scientist in his chosen career, he will find an organized setting more dependable and satisfying than self-employment in spite of financial limitations that either type of placement may entail.

These principles, among others, directly concern applications of psychology with respect to our society and our science. I feel, therefore, in the first place that we should not be indifferent today toward facilitating applications where we can. And further, that an organized or structured plan for furthering applied psychology will have most to offer in terms of continuity, of analysis and specialization, of achieving modifications in existing organization when required, and of personal security and satisfaction for those psychologists who elect to work in applied fields.



## PERFORMANCE VECTORS AND THE UNCONSCIOUS

BY

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*(Professor Tolman was introduced by Walter R. Miles,  
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The task of psychology, as I conceive it, is to explain and predict performances. And by performance I mean docile, goal-directed changes of the position of an organism with respect to external or internal stimulus objects. Further, these performances or "acts" (as they have been called by Guthrie to distinguish them from mere "responses" or "muscle movements") fall, I believe, into a few basic categories. These are: consumption; approach; escape; avoidance; and exploratory scanning. I am arguing, in short, that, in spite of the many qualitative, phenotypically different, kinds of acts which we see when we superficially observe mammalian behavior, either human or subhuman, all such different-seeming acts really reduce to one or more of the above five types.

A possible misunderstanding must, however, be guarded against. That is, it must be emphasized again that I do not include under this rubric of acts, or performances, the detailed movements per se, such as runnings, walkings, bar-pressings, vocalizations, etc., considered separately and divorced from their performance consequences. These movements and also such relatively pure reflexes as the knee-jerk, the salivary reflex, the galvanic skin response and other reflexes of the autonomic system may often be pointer readings for, components or accompaniments of, performances, but are not themselves performances. A complete psychology must, of course, explain all such movements and reflexes. But this wider explanatory problem lies outside of the present argument. Performances or acts are mediated by movements and accompanied by reflexes but they are defined and specified only by the organism-environmental rearrangements which they achieve.

Some years ago Ritchie and Kalish did a series of experiments with a simple, elevated T-maze which was provided with two alternatively usable, perpendicularly opposite, starting stems. Their results indicated that rats

could more easily learn to go towards the same side of the room—which ever stem they were started from—than they could learn always to make a righthand turn which would lead to one side of the room from one starting stem and to the other side of the room from the other. Further, even if finally taught always to make righthand turns (by having the food shifted appropriately) the animals tended to make these turns by varying movement details on successive occasions. To sum up, the only way in which one could sensibly specify *either* final behavior was to say: that, in the one case, the rats acquired the “performance” of turning down the same arm of the T and going towards the same side of the room, and thus getting to the goal box and food; and that, in the other case, they acquired the “performance” of always turning toward their own righthand sides and so getting to food. In both cases the character of the final performance could be correctly identified only from observing the results of at least two test trials—i.e., one from each of the two starting stems. That is to say, after a given performance was acquired, either that of turning to the right or that of turning towards the same end of the room, it appeared on every trial. But it could be identified and labelled with certainty only by observing it on at least two test trials, one from each starting stem. And this need for observation of two or more test trials for correct identification seems to be a requirement necessary for the specification of any performance.<sup>1</sup> Thus, for example, it would take quite a number of tests under varying starting circumstances to determine whether a type of prohibition habitually imposed upon a child by a parent was on each trial the performance of “protecting the child from undesirable consequences” or whether it was, rather, that of “aggressing against the child”.

Consider now the question of causal determiners. It is my thesis that any higher organism (e.g., rat, dog, cat, hen, pigeon, monkey or man—to cite those higher species with which we psychologists seem to be alone familiar) carries around with it (partly as a result of innate constitution and partly as a result of previous training) three distinguishable groups or quivers-full of dispositions. These three types of dispositions may be called discriminatory dispositions, expectancy dispositions and incentive-demand dispositions. And a specific performance on a specific occasion is to be conceived as the result of the activation then and there on that occasion from among these possible dispositions of one or more specific discrimina-

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<sup>1</sup> I wish to acknowledge here my indebtedness to Professor RITCHIE for this notion that more than one test trial is necessary for the specification of a performance (private communication).

tions, specific expectations, and specific incentive-demands which, taken together, form on that occasion a relatively unified and unitary discrimination-expectation-incentive-demand process.

But to return for a moment to the dispositions. By an organism's discriminatory dispositions I mean that, whatever stimulus objects be presented, then, because of this organism's innate and acquired endowment, there are only certain categories and dimensions according to which such stimulus objects can or will be discriminated. By expectancy dispositions I mean further that, however these immediate stimulus objects be discriminated, there will tend to be a certain group of sets (i.e., "expectancies") (based primarily on past learning) which will operate in the organism so that it, the organism, will be ready, if it should make such and such responses to these sign stimulus objects, for the coming of of such and such further significant stimulus objects. Finally, by incentive-demand dispositions I mean a group of innate or acquired dispositions possessed by the organism such that, given such and such deprivation conditions or such and such special stimulating conditions, it, the organism, will endow certain specific types of objects with such and such degrees of positive, negative or neutral incentive values or valences or (to use Lewin's original German term) with such and such degrees of positive, negative or neutral "Aufforderungscharaktere").

To sum up, then, a performance will be the result of a single unitary discrimination-expectation-incentive-demand process, which process will be determined both by the groups of discriminatory, expectancy and incentive-demand dispositions which are in the organism's repertoire and by the specific stimulus and deprivation conditions of the particular situation.

I shall not here attempt to elaborate this explanatory system further. The ways in which these postulated intervening dispositions and the then-and-there activated unitary processes made up of discriminations, expectancies, and incentive-demands are to be operationally (i.e., experimentally) specified will be discussed in detail in a book now in progress. It will have some such title as *Principles of Purposive Behavior*.<sup>2</sup>

Turn now to my choice of a title—"Performance Vectors and the Unconscious". The term "performance vector" is simply another name for what I have just called an activated unitary process of discrimination, expectancy, and incentive-demand. Such an activated process, or per-

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<sup>2</sup> I wish to acknowledge my great indebtedness to Professor LEO POSTMAN for his unstinting and crucial help in the development of this book.

formance vector, constitutes a tendency to a specific then and there performance. Thus, if there be no blockage from the environment or interaction from other vectors, a given performance vector will simply issue into its own corresponding actual performance. However, and this is the central point of my present interest, there are often situations in which a number of performance vectors must be conceived to be aroused simultaneously. And, when this happens, the final overt performance (i.e., the actual observed consumption, approach, avoidance, escape, or exploratory scanning which will appear) will be the result of some type of interaction between all the then-and-there simultaneously aroused performance vectors. It is the rules or principles of such interaction in which I am here interested. I must admit to having only vague notions about many of them. There are, however, a couple of such rules or principles which I do wish to propose. And they are the ones which, it seems to me, have a bearing upon the unconscious.

You may wonder that I, as a behaviorist, dare to refer to the unconscious implying also, as it does, the conscious. "Conscious" and "unconscious" are supposed to have become taboo words for us behaviorists ever since Watson. Well, in the first place, I am not a good, Simon-pure behaviorist. I am merely a pragmatic one. I am ready to grant you the fact of your immediate experience or (if you prefer the term) of your immediate phenomenology. I also grant myself the fact of an immediate phenomenology. But I consider such immediate phenomenologies, either yours or mine, as merely the initial experiential matrices out of which all sciences, physical as well as psychological, develop. These immediate experiential matrices do not in themselves, however, usually provide at first hand, good, scientifically useful constructs. Thus the distinction between conscious awareness and lack of conscious awareness is, I would feel, primarily a phenomenological distinction. As such, it is a starting point for science. But I feel that the later development of those further non-experiential explanatory constructs of "the" conscious and "the" unconscious has not perhaps been altogether helpful. And I am going to try to point out that some of the behavioral facts which have usually been discussed in terms of such concepts as "the" conscious and "the" unconscious can equally well be explained and perhaps better so in terms of my notions concerning the interaction between performance vectors.

I would suggest first that a major point we are interested in is the discovery and specification of those performance conditions which favor relatively rapid shifts in the dimension of discrimination of a sign or of a

significate or which favor rapid major shifts in the general type of an expectancy as contrasted with those conditions which do not favor such shifts. And I would also suggest that the conditions which favor such rapid shifts are ones which also tend to induce exploratory scanning performances, that is, behaviors such as looking back and forth (often called vicarious trial and error or VTE). Such scanning performances consist in responding to an initial sign stimulus situation by a scanning performance, the resulting significate produced by this scanning being the same stimulus situation but discriminated in some new way, either more precisely or along some new dimension. Thus a rat after having chosen pretty consistently the righthand door on the discrimination stand may shift (after a noticeable amount of new looking back and forth of VTEing—i.e., exploratory scanning), to jumping fairly consistently to the white door, whichever side the latter falls on. And the man after responding with, say, the words "circle of light" in the presence of a tachistoscopically presented small area of light may shift, (after perhaps a noticeable amount of new preliminary scanning) to responding with the words "pale yellow circle". It is these relatively sudden dimensional or precisional shifts (or the lack of them) in which we are interested. The rat, if he could talk (and thank God he can't) might say at such a stage in learning: "Aha, there are white and black doors and not merely lefthand and righthand ones". And the man in front of the tachistoscope might say, "Aha, I see it is not merely a circle, but a circle which is pale yellow". But this temporal correspondence between the appearance of such words and the appearance of such sudden shifts in the dimensions of discrimination would at the best be only approximate and, of course, would never occur in the rat. To repeat: what, therefore, I am interested in are the conditions which favor relatively rapid shifts in dimension or precision of discrimination and in types of expectancy and those which tend to interfere with such shifts and I am also interested in the presence or absence of coincident exploratory scannings. What now about my rules or principles as to performance vector interactions which could explain the appearance or non-appearance of such shifts and coincident exploratory scannings? I shall here suggest only two such rules.

- 1) The first would be that a strong pragmatic performance vector (as distinct from an exploratory scanning performance vector), whether this pragmatic performance vector be one of consummation, approach, avoidance, or escape (its strength due either to past frequency or to the magnitude of the incentive value on the significate or to both), will tend to prevent new exploratory scanning vectors and consequently tend to

prevent further shifts in discriminations and expectancies. The organism will (as long as all goes well and even for a surprisingly long time afterwards) tend to behave to all the features of the situation in the same old manner.

As a first example of this rule let me cite an early experiment by Krechevsky and Honzik. They found that the more their rats were overtrained in an original setup, which provided both a longer and a shorter path to food, the longer it took the rats to shift if the long path was then made the shorter and the short path made the longer. That is, if the interchange between the two paths were made just after the animals had reached the criterion of no errors in the original setup, the rats would shift almost at once. But, if the rats had been really overtrained in the original setup, it took a very long time for them to shift. In other words, when as a result of much overlearning a given performance vector had been made very strong, it interfered with new exploratory scannings and hence with the development of a new more appropriate set of discriminations and expectancies.

A second illustration of the same principle is to be found in some of the so-called "latent learning" experiments. In these experiments rats are run on a single T-maze or Y-maze. Thus, in an initial training period, the animals learn, say, while thirsty to go to the water side. They are, however, forced an equal number of times during this period to go down the other side which leads to food. In the next period the animals are made hungry. Will they show that they had discriminated the food and its location with respect to the choice point on the previous thirsty trials so that then later, when hungry, they will immediately go to the food side? The experimental answer in many, though not in all, cases has been "no". In short, it would appear that when the animals, in the first period, were developing a strong performance vector to go to the water side, this interfered with activations of the appropriate scannings and consequent additional discriminations and expectancies necessary for the development of a pragmatic performance vector with respect to the food. The already aroused strong approach-to-water performance vector tended, as I have put it elsewhere, to narrow the rat's "cognitive maps".

Thirdly, a recent experiment by Bruner, Matter and Papanek demonstrates the same point in still a different sort of setup. They used a discrimination box with four successive compartments, similar to the one originally devised by Krechevsky. The rat on each trial entered four successive compartments on the way to the food box and in each compartment it was faced with two doors, one at the left and one at the right.

One door of each pair was white and one was black. The white and black doors were randomized from side to side on different trials. The rats were first taught a white-black discrimination (i.e., always to go to the white door or to the black door; the other being locked). One group learned this under 12 hours of food-deprivation and a second group under 36 hours of food-deprivation. Each of these two groups were further divided into subgroups, one having 30 original trials and one having 100 original trials. After the first learning, in which all four groups had more than acquired the white-black discrimination, all four were then run for 20 trials more. In these 20 trials the white and black doors were still correct or incorrect, respectively, but in addition the correct door (white or black) was placed in a consistent left-right-left-right or right-left-right-left pattern. The question was would the animals now pick up a spatial alternation performance vector in addition to the white-black performance vector. This was tested in a still further series of runs in which all doors were gray but the spatial alternation of correct and incorrect retained. As the authors predicted, and as I would also have expected, those experimental animals, which showed that they had picked up the most alternation, were the less hungry, 12-hour deprived, group which had had only 30 original trials. And the experimental animals which picked up the least alternation (hardly any in fact) were the very hungry, 36-hour deprived, group which had had 100 original trials. Both excessive hunger and an overdose of original learning caused a strong pragmatic white-black performance vector which interfered with the scannings and resultant new discriminations and expectancies necessary for the development of a simultaneous spatial alternation performance vector.

One especially significant finding in the Bruner, Matter and Papanek experiment for my argument concerns the VTE's, or lookings back and forth at the choice points, during the 20 trials in which the alternation was introduced along with the white-black. The VTE's, or lookings back and forth from one door to the other before making a choice, were less for the 36-hour hungry rats than for the 12-hour hungry rats and less for the overtrained 100-trial rats than for the less trained 30-trial rats. In other words, the conditions of over strong motivation and over much previous training which interfered with the acquisition of the alternation discrimination were also the ones which produced less VTE, less looking back and forth, i.e., less exploratory scanning.

Now before concluding, I want to suggest a second principle.

2) This principle has two components. (a) Whenever there have developed both approach and avoidance performance vectors toward one



and the same general stimulus situation this will be a condition which will favor a then-and-there immediate exploratory scanning and good discrimination of the components of that situation. For the organism will be held in front of the situation ready to both approach and avoid it. Scannings and good discriminations, good immediate recall and good expectations relative to the details of the situation will therefore, I believe, be facilitated. (b) But, after a passage of time, the general approach vector relative to this whole situation will become weaker faster than the avoidance vector. (Conditioning experiments have shown that conditioned avoidance responses are more permanent over time than are conditioned approach responses.) And hence at such a later time the conditions will be unfavorable for good discrimination or for recall of the details of the situation. A long past situation which involved both general approach and general avoidance vectors will lose its discriminatory details. Now scannings and enhanced, improved, discriminations and expectancies upon the reinstatement of this situation will be less likely to occur. Thus on the basis of this opposing vector hypothesis, I would expect better discrimination and better immediate recall at or just after a moment of a conflict but poorer discrimination and recall at some later time. And this latter fact would be my substitute (no doubt, you will feel it an over simple one) for the concept of repression.

Actually, however, there is already some experimental (in addition to clinical) evidence which seems to fit in well with this notion.

Miss Babladelis, a graduate student now at California but doing the experiment at Michigan, found, using the Blacky test, that stories about those pictures which gave rise to conflict were better recalled in an immediate memory test but more poorly recalled after a time interval than were the stories about the non-conflictful pictures.

Secondly, there is considerable evidence from experiments on the Zeigarnik effect that, whereas memory for the interrupted tasks, which I assume to have been made conflictful by the interruption, is better in immediate recall, it is worse in delayed recall than is memory for the completed tasks.

Third, Postman and Rau, working at California, have found that in learning nonsense syllables those syllables which took the longest for original learning and hence were perhaps the more conflictful in the sense of involving more incompatible response tendencies, were relatively better retained in an immediate recall test but relatively poorer retained in a delayed recall test.

Finally, we are now doing some discrimination experiments at Berkeley



with rats which suggest, as yet only tentatively, that the introduction of shock in front of the choice cards, while in general it speeds up learning, is not so favorable when the trials are spaced, and there is thus a chance for the action of long time intervals to weaken the approach vector relative to the avoidance vector. These longer intervals between trials mean in the case of the shocked group that what had been learned in one trial cannot function as well 24 hours later at the beginning of the next trial.

But what now finally, you may ask, besides the introduction of some fancy new phrases such, say, as "performance vectors" and "exploratory scanning performances" have I contributed in this brief talk? You may well answer: "Nothing", or at any rate, "Nothing new". However, it does seem to me, that the following points have been made and I wish in summary to emphasize them:

1) The study of behavior will be most fruitful when behavior is conceived as consisting in performances and not in mere movements or responses per se.

2) Performances depend upon the discrimination, expectancy, and incentive-demand dispositions which the given organism has innately or has acquired and upon the immediate activation by virtue of the then-and-there present stimulus and deprivation conditions of specific complexes of discrimination, expectancy, demand, and incentive value. These activated complexes I have conceived as performance vectors.

3) These performance vectors interact according to certain rules or principles.

4) I have suggested two such principles: a) When a pragmatic performance vector has become very strong either because of over learning or over motivation, or both, it will interfere with new scanning performances and interfere with new discriminations and expectancies which could lead to a later more appropriate pragmatic performance vector. b) A conflict between an approach and an avoidance performance vector to one and the same total stimulus situation will lead to good immediate discriminations, expectancies and recall, but to poorer later discriminations, expectancies and recall. Thus, whereas it has been customary to say that a conflict causes the initial stimulus situation to be "repressed" into "the" unconscious, I prefer to say merely that after a long enough time interval the general approach vector towards the original conflict situation tends to become relatively weaker than the avoidance vector away from that situation. And this leads to poorer later recall and discrimination.

In conclusion, my practical slogan would be, then: let those of us who are primarily experimental psychologists seek to do more studies of such

simple conflict situations as we can under controlled laboratory conditions, using as subjects both rats and human beings. Such studies have of course already been initiated by Liddell, Masserman, Maier, Neal Miller, Mowrer, and Lewin and his students. I urge, merely, that those efforts must be continued and expanded. And, I argue that eventually the concept of interactions between performance vectors will provide a more useful explanatory scheme for explaining the behavioral results of such conflicts than will the concept of "the" unconscious and the to-my-mind rather mystical properties which have often been assigned to the latter.

One last word. Professor Hebb here at McGill has made the concept of expectancies neurologically respectable. I can only pray that in the future he will also be interested in trying to make that of performance vectors and their interactions likewise respectable.

EVENING ADDRESSES — CONFÉRENCES DU SOIR

TUESDAY JUNE 8 — MARDI LE 8 JUIN

*Chairman — Président*

OMER-JULES DESAULNIERS

*Surintendant, Département de l'Instruction Publique de la Province  
de Québec*

PERCEPTUAL AND COGNITIVE (OR OPERATIONAL)  
STRUCTURES IN THE DEVELOPMENT OF THE CONCEPT OF  
SPACE IN THE CHILD

BY

JEAN PIAGET

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*(Professeur Piaget was introduced by Paul Fraisse,  
Professeur à l'Institut de Psychologie de la Sorbonne)*

*(Editor's note.—In conformity with Professeur Piaget's request, this abstract is presented in English. The original address has been given in French.)*

*(Note de la rédaction. — Pour nous conformer à la demande du Professeur Piaget, nous présentons en anglais le résumé de cette conférence qui a été donnée en français.)*

Our problem here consists in determining how the child succeeds in elaborating the concept of a Euclidean, homogeneous and isotropic space, and also that of a projective space with its rules of perspective (with a reciprocal and other characters). Does perception alone allow the child to achieve this end or are cognitive systems of an operational nature necessary? This is a problem we have investigated in the child by means of a genetic approach.

Let us first give our attention to perceptual space.

## I. PERCEPTUAL SPACE

1) There seems to exist, at first sight, a number of analogies between perceptual space, in particular visual space, and cognitive or operational space. Perceptual constancies herald the operational forms of "conservation", geometrical "good Gestalten" are related to cognitive Euclidean structures, and projective vision seems to prepare an understanding of perspectives.

2) Perceptual space however is not of a Euclidean nature, at least in *field effects*, where it is neither homogeneous nor isotropic. Helmholtz's checker-board image already raised such a problem; a mathematical solution of this was recently proposed by Lunenburg.

3) A very simple hypothesis, bearing on the heterogeneity of visual space can be put forward for the simultaneous explanation, in the sphere of "field effects", of effects of contrast and of equalization (with the thresholds of equality which are found during experimentation). We have been led to admit that when elements are compared they are alternately or simultaneously overestimated by a constant coefficient  $p > 1$ . This leads to the following results where  $A$  and  $A'$  are the corresponding lengths of the elements compared:

- a) If  $pA > A'$  and  $pA' < A$   
then  $(pA - pA') > (A - A')$

and we have an effect of *contrast* between the elements compared.

- b) If  $pA > A'$  and  $pA' > A$   
then  $pA \geq pA'$

and we have an effect of equalization of the elements compared.

4) A general quantitative formulation was drawn from this <sup>1</sup>. It allows the explanation of the *maxima* and *minima* of most geometrical illusions, such as the illusions of Delbœuf, of rectangles, of divided lines (Oppel), of angles and of curves. We have studied these illusions through comparisons, when one element of the figure is systematically varied. If we call  $L_1$  and  $L_2$  respectively the larger and smaller of two lengths which are compared to each other, the illusion  $P$  which sets in is expressed by the formula:

$$\pm P = \frac{n(L_1 - L_2)(L_2/L \max)}{S}$$

<sup>1</sup> See the Proceedings and Papers of the XIIIth International Congress of Psychology, Stockholm 1951.

where  $L_{max}$  = the maximum length of the figure

$S$  = the surface of the figure

$n$  = the number of times the ratio or difference intervenes.

5) The explanation given for these relationships is that when an element of a figure is fixated by the eye, the centered part is overestimated and the periphery underestimated. Experimental controls of this phenomenon are in course. Let us here merely mention the well-known fact that in field effects, visual space is not homogeneous, but undergoes a continual process of expansion or contraction, according to the point fixated by the eye. The effects of contrast or equalization mentioned in 3) proceed directly from this.

6) If we consider "good Gestalten" which belong in part to the sphere of field effects (perceptual activities intervene here also as we shall see later), we find they are not free from this phenomenon. But here compensations attain their maximum value. In the square for instance a comparison of two equal sides  $A$  and  $A'$  lead to the effect  $pA - pA' = 0$ . The principal character of a "good Gestalt" is that compensations are here at a maximum.

7) Experimental evidence shows a progress of perception with mental development. To account for this, we must resort, beside field effects which are relatively stable during the development, to a certain number of perceptual activities, such as exploration of the figure, decentration of the figure through co-ordination of successive partial decentrations, "carrying over" of an element upon another at increasing distances, both spatial and temporal, with age, transpositions, anticipations, etc. These perceptual activities develop with age.

8) An experimental study has shown that the resistance of good Gestalten themselves increases with the age. Following an idea of Rubin's, we used a figure where a square is combined with a Müller-Lyer illusion, and we measured the illusion thus obtained at different ages, from which we deducted the value of the Müller-Lyer illusion alone. We find that the good Gestalt is less resistant or more elastic at the ages of 5 to 7 than at the ages of 9 to 10 or in the adult. This is due to the fact that at 7 to 9 years of age, contrary to what happens at 5 to 7 years, the secondary effect of "carrying over" and others are added to the primary field effects. Good Gestalten are then found to be three times more resistant at 9 to 10 than at 5 to 7 years of age.

9) The study of perceptual constancies at different ages also shows that, to a period between 4 and 7 years, where there is a medium under-

estimation of sizes at a distance, succeeds a period of "over-constancy" after 11 to 12 years of age, when sizes at a distance are slightly over-estimated. This is due to an active mechanism of regulation, or perceptual activity, which appears with age.

10) Perceptual activities are themselves the source of secondary illusions which increase slightly with the age. Such are the error of the vertical line, found in older children jointly with a better structuration of space at greater distances, the error which appears in the estimates of projective size due to the growing importance of objective versus projective constancy in the adult, and certain temporal errors.

Thus we find that perceptual space is not homogeneous in the young child because of errors due to centration. In the course of development, a gradual process of correction sets in, due to mechanisms of perceptual activities. Correction however never leads to a total precision. We do not find in perception the source of a homogeneous isotropic, Euclidean space.

## II. COGNITIVE OR OPERATIONAL SPACE

1) The cognitive or representational concept of space develops along quite different lines than perceptual space during the three principal stages of child development. We had been led to distinguish the following stages: that of pre-operational representation from the ages of 2 to 7, that of concrete operation from 7 to 11, and that of formal operations after 11 to 12 years of age.

2) At the *preoperational level* (2 to 7 years), both the study of children drawings and experimental evidence show that the situation is the following:

a) There exists an initial form of representation which is neither Euclidean nor projective, but topological. The child is interested by open or closed figures and the situation inside or outside a border-line.

b) Projective representation is absent and perspective is not understood.

c) Euclidean good Gestalten appear from the ages of 4 to 5 onward, but without conservation of lengths, distances or surfaces.

d) There is no conservation because the representation of the physical world depends closely upon its perception. The non-conservation of distances is due to the fact that a different importance is given to full and empty spaces. This is evidenced in the Oppel illusion.

e) The child's reasoning does not bear on a system of transformations but on actual states.

3) At the beginning of the stage of *concrete operations* (from the age of 7 onward), the child succeeds in mastering:

- a) a system of reversible operations, such as partitioning and reuniting, placing and displacing, and measuring,
- b) the conservation of lengths, distances, and certain surfaces,
- c) certain elementary projective operations, such as the point to point construction of a projective straight line, and elementary perspective.

4) At the end of this stage of concrete operations (11 years), the child has constructed:

- a) a natural system of reference, including an understanding of the horizontal and vertical lines,
- b) the co-ordination of the various perspectives possible up on the same scene.

5) At the stage of *formal operations* (after 11 to 12 years of age), the child succeeds in mastering the concept of volume, that of proportions, etc.

Thus experimental data show that the origin of Euclidean space such as evidenced in our concept of the physical world finds its source in the cognitive approach to reality. This develops gradually with the age.

### III. THE RELATIONSHIP BETWEEN PERCEPTUAL AND OPERATIONAL SPACE

1) A comparison between the development of perceptual and cognitive spaces in the child shows that we must distinguish the two approaches to space. It is impossible to consider the operational concept of space as simply drawn or abstracted from perceptual space. A number of situations leading almost to paradoxes would remain unexplained. Let us here insist upon three instances where an opposition is found.

a) The understanding of the concept of perspective begins around the ages of 7 to 8 and does not influence the drawings of a child until around the ages of 9 to 10 in the average. Now the perceptual estimations of apparent size (and not of true size) is far better in young children of 6 to 7 years of age than in the adult (with the exception of painters); the maximum error lies around the age of 10. We have measured with Lambercier the various possibilities of estimate by asking subjects to compare the projective sizes of vertical rods set at 1 and 4 meters from them. The curve of cognitive attainments is here independent from that of perceptual performances.

b) The horizontal and vertical lines play a part early at the perceptual level; precision increases slightly with the age. At the cognitive level

however, it is only around 9 to 10 years of age in the average that the child can foresee the horizontal surface of water in an inclined bottle or the vertical slope of a plumbline which is not parallel to its frame of reference.

c) Last, but not least, the perceptual constancies of size and shape appear early (at 5 to 6 and 11 to 12 months respectively). Precision increases slightly with the age. Yet it is only around the ages of 7 to 8 that operational concepts of conservation are formed. At this age, two short rods which seem to be equal when placed at the same level, remain so even when slightly shifted. This was not the case earlier. The distance between two objects, an empty distance, remains equal at this age when a screen is set between the objects, contrary to what happened earlier. The surface of two figures remains the same when the figures are modified in a certain manner, which it did not earlier. And so on. Thus operational conservation, it is found, has no direct relationship with perceptual constancy (notwithstanding the existence of intermediate situations like constancy in the extended sense of E. Brunswik).

2) Now what makes the difference between these two approaches to space? What is added to perception during the elaboration of a cognitive space is a system of *operations*. By this we mean a system of internalized actions, which are co-ordinated into reversible structures. These operational structures, the principal character of which is an additive composition, cannot be reduced to the structures found in the perceptual field, the "Gestalten" which are distinguished by a non-additive composition.

3) Ever since the onset of the sensori-motor level the part played by action increases to the prejudice of that played by perception. It is in action that we find the starting point of future operations. Now action also influences perception.

a) The co-ordination of prehension and vision (which appears between 4 and 5 months) precede the elaboration of the perceptual constancy of size (which sets in around 5 to 6 months).

b. The elaboration of the thing-concept influences the constancy of shape.

c) The "group" of displacements constitutes a fundamental type of structure on which are built the future operations.

4) From the ages of 9 to 10 onwards perceptual activities themselves are guided by operations.

5) As a conclusion, we shall say that geometrical space is not a perceptual continuum. It is an operational continuum due to the co-ordination and internalization of actions.



WEDNESDAY JUNE 9 — MERCREDI LE 9 JUIN

*Chairman — Président*

E. LOWELL KELLY

*Professor of Psychology, University of Michigan*

THE PERMANENT RECORD OF THE STREAM OF  
CONSCIOUSNESS

BY

WILDER PENFIELD

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Director of the Montreal Neurological Institute*

*(Professor Penfield was introduced by Roger W. Russell,  
Professor of Psychology, University College, London)*

While considering how I might fortify my position before this Congress, composed as it is of leading psychologists from all the world, I took from my book shelves the two volumes of Psychology by William James (1). I blew off the dust that had lain upon them, I fear, since my undergraduate days at Princeton and read his classical chapter on The Stream of Thought.

Consciousness, he said, is a personal phenomenon. It deals with external objects, some of which are constant, and it chooses among them. But consciousness is never the same in successive moments of time. It is a stream forever flowing, forever changing.

It has fallen to my lot, during explorations of the cortex, to demonstrate a mechanism in the human brain which preserves the record of the stream of thought. When it becomes necessary to operate under local anesthesia and to stimulate the surface of one of the temporal lobes, it happens occasionally that small parts of that record are activated, bringing back a period of past experience with a startling degree of vividness and detail. The patient then re-views the sights and sounds and thinking of a previous period of time.

During the past twenty years of neurosurgical practice it has been necessary to expose the cerebral cortex under local anesthesia in a succession of patients. The reason for operation was that they were afflicted by recurring attacks of focal epilepsy. Surgical excision of an abnormal area of cortex in which epileptogenic discharges arise may relieve such patients of their attacks in about fifty percent of cases.

But before excisions are carried out, it is our custom to explore the exposed cortex, applying a gentle electrical current to it from place to place. It is possible, thus, at times, to reproduce the beginning of a patient's attack in the form of a sensory aura or movement and, thus, to verify the position of the epileptogenic focus. Furthermore, stimulation is used to localize the functional areas of the cortex after which the excision can be carried out with a minimal sacrifice of function.

This practice made it possible to map out, in great detail, the motor representation in the cortex of man and the sensory areas—somatic, visual, auditory, etc. As years passed the supplementary motor area was demonstrated, hidden away in the sagittal fissure, and the second somatic sensory area within the fissure of Sylvius.

Two major areas of the human cortex remained, the anterior half of the frontal lobes and the enormously in-folded temporal cortex.\*

From time to time over the years, stimulation has produced an astonishing result. Instead of movement or sensation, application of the electrode has caused the patient to report a psychical effect.\*

In each case the location of stimulus was charted and a description immediately dictated to a secretary who sat in the "viewing stand" for the purpose. In such dictation the surgeon has always used the patient's own words as far as possible.

Positive psychical responses were obtained only from the previously unclaimed cortex of the temporal lobes and only then when that cortex had been rendered more responsive by long continued pre-operative

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\* A relatively small part of the first temporal convolution, the transverse gyrus of Heschl, was clearly devoted to auditory sensation and, perhaps adjacent to it, was some representation of labyrinthine sensation. The sense of smell has been given an ever smaller foothold in the region of the uncus. Otherwise the temporal cortex, which has enlarged so greatly in man, as compared with other mammals, represented a vast and unclaimed territory.

\* It may be said at once that the word psychical is used in its original meaning, as Hughlings Jackson used it, to denote the more complicated mental phenomena made possible by final neuronal integration within the brain.

epileptic conditioning. The responses, however, might bear no relationship to the content of the patient's seizures and they were produced without setting up epileptic after-discharge. Sensitization of the cortex, so that it reveals its function with greater ease when stimulated, is the common result of the presence of local epileptogenic process in other areas of cortex as well as in the temporal lobes. The stimulating electrode may imitate the effect of a local epileptic discharge but it does other things as well.

#### GENERAL STIMULATION RESPONSES AND THE MEANING OF CORTICAL LOCALIZATION

It was in 1870 that Fritsch and Hitzig applied an electrical current to a certain area of the cerebral cortex of a lightly anesthetized dog and produced movement in the opposite limbs. Previous to that time, the brain was considered to be the organ of the mind functioning somehow as a whole, although it is true that Hughlings Jackson had suggested some degree of localization and Broca had claimed a localized representation for speech.

The work of Fritsch and Hitzig, followed quickly by Ferrier, prepared the way for the great experimentalists to study the cerebral cortex. I refer to Sherrington and Pavlov and their pupils, but I should also mention many others, Luciani, Dusser de Barenne, Graham Brown, Adrian, Bard, Woolsey, Vogt, Lashley, to name only a few.

What can we say today about localization of function in the cerebral cortex of man? And in what sense can any function be said to have a localization? These questions have been discussed exhaustively elsewhere (2, 3, 4) with references to literature. But a few words of interpretation may be useful here.

Stimulation of the motor cortex of conscious men produces movement (Fig. 1) but the patient is always aware of the fact that he has not willed these movements. These movements are gross and uncomplicated with certain striking exceptions. The exceptions are motor performances which have a localization of mechanism elsewhere in the central nervous system, mechanisms which can be activated from a distance by the cortical stimulation. I refer to vocalization, mastication, swallowing, conjugate eye movements.

Stimulation of sensory areas causes the patient to experience only the elements of sensation. In the somatic area it is tingling, numbness, sense of movement of some part of the body; in the visual area, lights, shadows, colored forms usually moving; in the auditory area, a buzzing, humming, knocking or ringing sound.

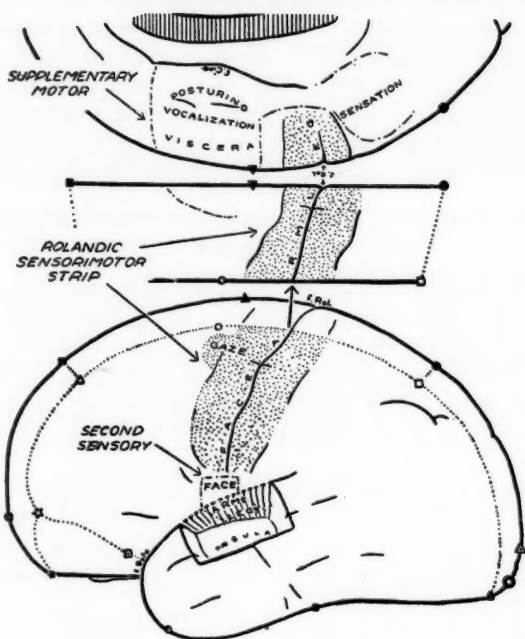


Fig. 1.

Somatic sensory and motor areas of the human cerebral cortex. As judged by the results of stimulation all three somatic areas (Rolandic, Supplementary, Second) are to some extent both motor and sensory. (From Penfield and Jasper, 1954.)

Sensation (Fig. 2) can not be said to be located in the cortex any more than it is in the peripheral receptor. Sensory areas of the cerebral cortex are way-stations in the current of afferent neuronal impulses. These impulses originate in the peripheral sense-organs and travel inward and upward to the cortex with ganglionic interruption in subcortical nuclei. From the cortex they pass inward again to the higher brain stem where the afferent stream from one field or body-half can join the others.\* The most important, and the final, reorganization of sensory material must take place in the circuits of the higher brain stem rather than in the cerebral cortex.

The motor pathway, on the other hand, which subserves voluntary

\* Interference with this portion of the brain produces unconsciousness (5).

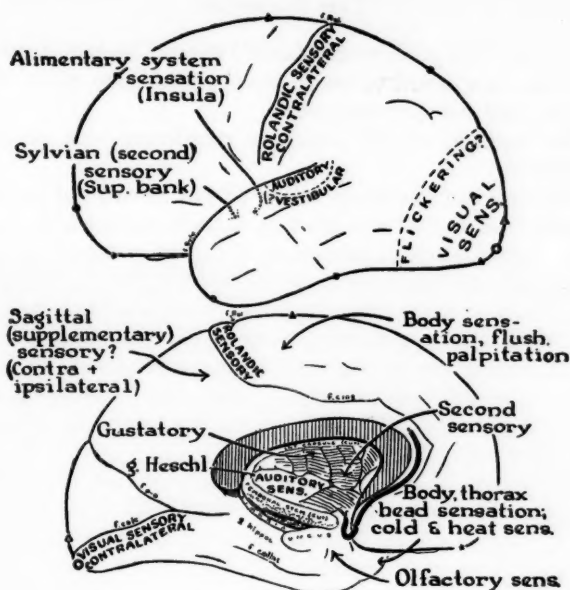


Fig. 2.

Sensory areas of the human cortex as judged by stimulation exploration in conscious patients. (The sensory area indicated in the anterior temporal region requires verification.)

discriminative movement originates in a central region between the hemispheres and passes out to the motor cortex of each hemisphere. After its ganglionic interruption in the precentral gyrus it descends to medulla and spinal cord and thence to the muscles.

Associative neuronal intercourse from one functional area of the cortex to another is of comparatively little importance. Integration of the function of the separate areas of the cortex by a subcortical centrencephalic system is of great importance.

It is the purpose of this address to discuss the ganglionic organization of an experiential recording mechanism, and to point out that there is some sort of representation of this mechanism in the temporal cortex. As time passes others will, no doubt, elucidate the nature of the role of the temporal cortex in this mechanism.

## CASE EXAMPLES

*Case T. S.* T. S. was a young man of 19 years. He had temporal lobe seizures that were sometimes precipitated by listening to music. He was fond of jazz and also symphonic music.

At the beginning of each attack he experienced what he called a "flash-back".\*

He explained that this usually had to do with himself and his past but was "much more distinct" than anything he could summon to his memory.

At the time of operation, stimulation of a point on the anterior part of the first temporal convolution on the right caused him to say, "I feel as though I were in the bathroom at school". Five minutes later, after negative stimulations elsewhere, the electrode was reapplied near the same point. The patient then said something about "street corner". The surgeon asked him, "where", and he replied, "South Bend, Indiana, corner of Jacob and Washington". When asked to explain, he said he seemed to be looking at himself—at a younger age.

When the stimulation was repeated the response was quite different. This time he said, "that music, from 'Guys and Dolls'." When asked which song in the play he referred to, he could not name it. "I was listening to it," he said. "It was an orchestration" . . .

Such results have been produced many times and we have used every practicable control and verification. The following case may be reported in greater detail.

*Case M. M.* The patient M. M. was a woman of 26 years who was afflicted by recurring cerebral seizures. The first manifestation of each attack was a sudden "feeling—as though I had lived through this all before". At times there was also a feeling of fear. On other occasions she experienced what she called a flash-back not unlike those just described in the case of T. S.

The initial feeling of familiarity she described as applying to the whole of any experience she might be having at the moment. On the other hand, the flash-backs were experiences from her earlier life. They came suddenly while she retained awareness of her actual surroundings. She gave the following example: Without warning she seemed to be sitting in the railroad station of a small town, which might be Vanceburg, Kentucky, or

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\* Flash-back is an expression used by those familiar with moving picture techniques to describe the presentation of a scene that has occurred in the earlier history of one of the characters of the play.

WILDER PENFIELD: *The permanent record of the stream of consciousness.*



Fig. 3.

Prepared for operation. Incision marked on scalp after injection of nupercaine.  
Case M. M.

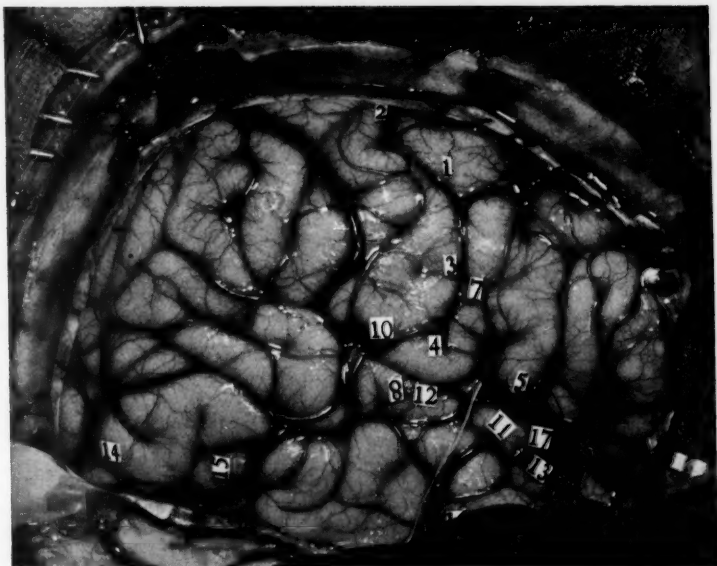


Fig. 4.  
 Photograph of brain after craniotomy. The paper tickets dropped on the cortex show points where stimulation produced positive responses, sensory, motor, or psychical. Case M. M.



Fig. 6. Left hemisphere exposed at operation. Case N. C.



perhaps Garrison. "It is winter and the wind is blowing outside and I am waiting for a train." This was apparently an experience from her earlier life but it was one she had "forgotten".

These minor seizures (psychical seizures) were often followed by automatism, periods of irresponsible behaviour of which she would have no memory. During these periods she might fall or walk about in a confused state, speaking unrelated and disjointed words and sentences.

Thus, in summary, the localized epileptic discharges in the right temporal lobe of this young woman were causing her to experience, from time to time: 1) a sense of false familiarity (*déjà vu*), 2) a feeling of fear, 3) reproductions of previous experience.—The first was an illusion, the second an emotion, the third an hallucination. These are all to be considered psychical phenomena, any one of which the operator might hope to reproduce by stimulation.

Osteoplastic craniotomy (Figs. 3 and 4) carried out under local anesthesia disclosed atrophy and sclerosis of the first temporal convolution, and the uncus and hippocampus as well. Electrographic recordings taken directly from the cortex by my associate, Dr. Herbert Jasper, showed spontaneous "spike" discharges from this area.

Electrical stimulation was carried out (Square wave generator, 60 cycles, 2 millisecond pulses). She was ordinarily warned by the operator each time the electrode was applied. But, as usual, at intervals the warning was given with no stimulus and at other times, stimulation without warning. This serves to eliminate with certainty false or imaginary responses.

### *Sensory and Motor Responses*

A current of two volts proved to be the minimum threshold strength that would produce responses from the sensory and motor areas. See Figs. 4 and 5.

Stimulation at point 2—sensation in thumb and index finger. She called it "quivering", "tingling".

3—"Same feeling on the left side of my tongue."

7—Movement of the tongue.

4—"Yes, a feeling at the back of my throat like nausea."

8—She said, "No." Then she said, "Yes, I suddenly can not hear." This is, obviously, the border of auditory sensory cortex.

The foregoing responses were motor and sensory in character much like those obtained routinely. They serve only to identify the Rolandic sensory and motor cortex and Heschl's auditory gyrus, a part of the first temporal convolution buried within the fissure of Sylvius.

*Psychical Responses*

The following effects are psychical. The strength of current was increased from 2 to 3 volts.

11—"I heard something familiar, I do not know what it was."

11—(repeated without warning)—"Yes, Sir, I think I heard a mother calling her little boy somewhere. It seemed to be something that happened years ago." When asked to explain, she said, "It was somebody in the neighbourhood where I live." She added that it seemed that she herself "was somewhere close enough to hear".

Warning without stimulation—"Nothing."

11 repeated—"Yes, I hear the same familiar sounds, it seems to be a woman calling, the same lady." That was not in the neighbourhood. It seemed to be at the lumber yard." Then she added reflectively, "I've never been around the lumber yard much."

This was an incident of childhood which she could never have recalled without the aid of the stimulating electrode. Actually she could not "remember" it but she knew at once, with no suggestion from us, that she must have experienced it sometime. The same incident was evoked again by another stimulation at approximately the same point. Then at a different point, 12, she had another experience but of a similar character. The ticket 12 was displaced before its position was recorded.

12—"Yes. I heard voices down along the river somewhere—a man's voice and a woman's voice calling."

When she was asked how she could tell that the calling had been "along the river", she said, "I think I saw the river." When asked what river it was, she said, "I don't know. It seems to be one I was visiting when I was a child."

Warning without stimulation—"Nothing."

Three minutes later without any warning stimulation was carried out again, probably near 13. While the electrode was held in place, she exclaimed: "Yes, I hear voices. It is late at night, around the carnival somewhere—some sort of travelling circus." Then, after removal of the electrode: "I just saw lots of big wagons that they use to haul animals in."

These simple re-enactments of experience had auditory and visual elements in them.

Eleven minutes later, stimulation was carried out without warning at a point just posterior to 11—"I seemed to hear little voices then," she said, "the voices of people calling from building to building somewhere—I do

not know where it is but it is very familiar to me. I can not see the buildings now but they seemed to be run-down buildings."

14 (just posterior to 15)—This stimulation caused her to say: "The whole operation now seems familiar."

Warning without stimulation—"Nothing."

15—"Just a tiny flash of familiarity and a feeling that I knew everything that was going to happen in the near future." Then she added, "as though I had been through all this before and thought I knew exactly what you were going to do next."

At point 17, an electrode, covered with an insulating coat except at its tip, was inserted to different depths and the current switched on so as to stimulate in various buried portions of the first temporal convolution and uncus.

17c (1 cm deep)—"Oh, I had the same very, very familiar memory, in an office somewhere. I could see the desks. I was there and someone was calling to me, a man leaning on a desk with a pencil in his hand."

Warning without stimulation—"Nothing."

11 (forty minutes after first stimulation of this point)—"I had a flash of familiar memory. I do not know what it was."

13 (repeated three times)—"Nothing."

11 (after four minutes)—"Nothing."

Conditions seemed to have changed and stimulation now would summon no experiences.

The plan of surgical excision had now been formulated. Accordingly the second and third temporal convolutions were removed, exposing the first temporal convolution and the uncus and hippocampal gyrus deep within the temporal fossa.

Stimulation near uncus or just lateral to it, at 18a—"I had a little memory—a scene in a play—they were talking and I could see it.—It was just seeing it in my memory."

Stimulation at a point nearby—"I feel very close to an attack—I think I am going to have one—a familiar memory."

20 (Stimulation of the lateral aspect of the hippocampal gyrus)—"Oh, it hurts and that feeling of familiarity—a familiar memory—the place where I hang my coat up, where I go to work."

The patient M. M., described in this case, was a good witness, self-critical, understanding and tolerant. I have reported the features which are of psychological interest throughout the whole procedure of exploration

and crotical excision, a three-hour period. But little reference is made to the pathological, surgical and electrocorticographic details.

A zone of abnormality due to birth compression was found (shown by cross hatching in Fig. 5). It was obvious that years of epileptic discharge arising in this zone had sensitized the temporal cortex so that stimulation could produce psychical responses. Stimulations elsewhere in the lobe were without positive effect even when a small increase was made in voltage.

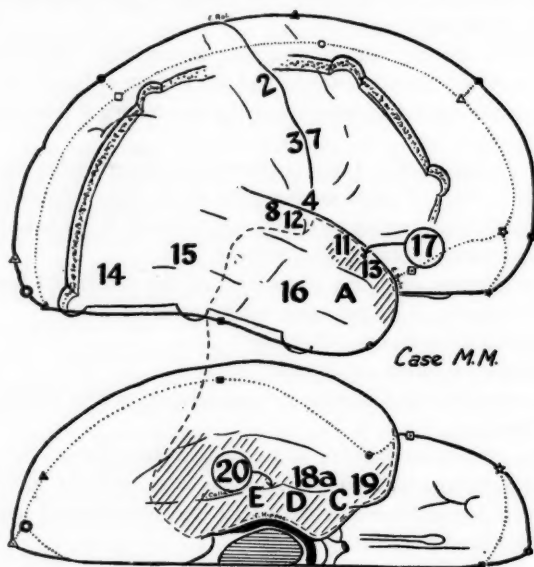


Fig. 5.

Diagram of the operation. Broken line shows extent of removal of temporal lobe in treatment of the focal epilepsy and the shading indicates sclerosis and atrophy due to arterial compression associated with herniation of the hippocampus through the incisura of the tentorium, at the time of birth (incisural sclerosis).

The psychical hallucinations, thus produced, were experiences from this patient's past, not particularly important ones, and not ones that she could voluntarily remember with anything like the clarity of the hallucination. Yet she accepted them as part of her own past and she confessed that she was present in them. They brought to her the strong sense of familiarity that means recognition.

*Case N. C.* (Figs. 6 and 7). This patient had seemed to be asleep on the operating table, but when the second temporal convolution on the left side was stimulated at point 19, she spoke as follows: "I had a dream, I had a book under my arm. I was talking to a man. The man was trying to reassure me not to worry about the book."

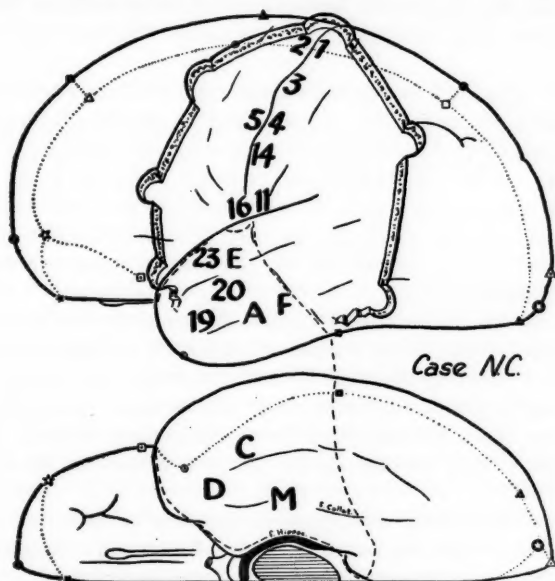


Fig. 7.

Diagram of operation. Case N. C.

At 20, one cm distant, she said: "Mother is talking to me." Ten minutes later, point 20 was stimulated again and she laughed aloud. When asked to explain she said: "Well, it is kind of a long story but I will tell you." The stimulating electrode was then withdrawn. It seemed the occurrence took place at her home in Richmond, Washington. She said to her mother, "Don't forget the fabulous feeling." Her mother "mocked" her, she said, and she concluded the story, "I took my arm and brought it down on the plate and broke it all to pieces."

Stimulation was repeated at point 20 (Fig. 7) without warning. While the electrode was kept in place the patient spoke quietly: "Yes, another experience, a different experience, a true experience. This man Mr.

Meerburger, he, oh well, he drinks . . . The electrode was withdrawn and the patient continued to explain her experience. I said, 'hmm, he is back', meaning Mr. Meerburger's little boy, and the lady said, 'What is the matter', etc. etc. etc." This was an experience she did remember.

At 23 on the first temporal convolution, stimulation caused her to hear music. Six times at intervals, stimulation was carried out here. She heard first a "baby song" and later, what she called "The War March of the Priests".

On one occasion following stimulation which had been carried out with no warning, she said, "Yes. I was trying to identify the song." The surgeon asked her if she would like to have him stimulate again and she said, "yes". After the electrode was reapplied and while it was held in place she was quiet for a time. Then she began to hum and she hummed an air quite accurately. Finally she said, "Yes, it is the War March of The Priests." It was obvious that she had been accompanying the music with her humming.

Four minutes later, as a final test, she was asked to report just as soon as she heard music. After considerable delay the electrode was applied again at 23. Instantly she said, "There." It was the "War March" again. She explained that it was an orchestra playing each time, without any voice. She added that she had a phonograph record at home with the "Hallelujah Chorus" on one side and the "War March" on the other.

*Case R. M.* During stimulation of the superior surface of the left temporal lobe within the fissure of Sylvius, R. M. said: "A guy coming through the fence at the baseball game, I see the whole thing." Afterward he said, "I just happened to watch those two teams play when the fellow came through the fence . . . That would be like the beginning of an attack, anything might come up." He went on to explain that such scenes from his past came to him suddenly at the beginning of a seizure, when he was thinking of something else, things he had forgotten all about.

One more example may be described. In this case the hallucination had to do with thoughts. It is difficult to discover whether in such cases the thought is divorced from any visual or auditory content or not.

*Case A. D.* This patient had temporal lobe seizures introduced by having what he called two thoughts simultaneously.

Stimulation in the first temporal convolution caused him to say, "My thoughts bounced together and I was mixed up for a second."

When the stimulation was repeated, he said, "The same two thoughts

came together." After the electrode was withdrawn he explained that one of the thoughts was concerned with what was happening at the present time and the second thought was different but he could not recall it clearly.

When the same area was stimulated after an interval of time he said, "This is it." When asked whether he had had a memory, he said, "No. It is the thought that crosses." But he could not explain and gave up the effort.

### DISCUSSION

The foregoing examples demonstrate the nature of evidence upon which this discussion must be based. I have published other cases elsewhere and shall draw on our total experience in this argument.

#### *Psychical Responses*

From the patient's point of view there is a great difference between psychical responses and sensory responses to stimulation. When a sensory area is stimulated the patient never seems to feel an object. He does not hear words or music, nor see a person or building. In sensory responses there are no recollections of the past and the subject himself is usually clear that the sensation is not an ordinary experience at all.

What we have referred to as psychical responses, on the other hand, include many different elements of thought, made up of auditory, visual, somatic and labyrinthine information, as well as interpretations, perceptions, comparisons, emotions.

Under the heading of psychical, there are two types of response. One is a reproduction of past experience and the other is a sudden alteration in interpretation of present experience. Thus the psychical responses to stimulation, taken together, may be divided into two groups:

1. *Experiential.* This has to do with the past and includes past events and past interpretation.
2. *Interpretative.* This has to do with the present.

#### *Experiential Responses*

When these flash-backs, these short reproductions of past experience, occurred as epileptic phenomena, Jackson called them dreamy states. They are the same when produced by stimulation—drawn from the patient's past experience. Let me use the words of the patient M. M. again: "I had a little memory—a scene from a play—they were talking

and I could see it", and again, "Oh, familiar memory, in an office somewhere. I could see the desks. I was there and someone was calling to me, a man leaning on a desk with a pencil in his hand."

All the detail of those things to which she had paid attention are still there. Perhaps the pencil in his hand had seemed important, but other images that must have reached her retina during the original experience are now lost for they were ignored originally. Throughout all of these evoked experiences she continued to be aware of the fact that she was actually in the operating room.

It is clear that a flash-back response is usually completely experiential, including events and also the patient's interpretation. The patient feels the attendant emotion and understands the original meaning.

### *Interpretive Responses*

As an example of interpretive responses, take again the case of M. M. When the electrode was applied to another area of the temporal lobe it produced a sudden sense of familiarity which she referred at once to her present experience. She felt the operation had happened before and that she even knew what the surgeon was about to do. This occurred independently of any recollection of the past.

When such interpretations have been described by temporal lobe epileptics, clinicians have long called them "déjà vu" phenomena. They are disturbances of the present process of interpretation. They are illusions, but these illusions take different forms. There may be a false sense of familiarity as already described; or, on the contrary, everything may seem strange or absurd. The relationship of the individual to his environment may seem to be altered. The distance from things seen or heard may seem to be increased or decreased. The patient may say he is far away from himself or from the world.

Allied to these altered interpretations is the production of emotions not justified by the experience. Fear is the commonest emotion produced by stimulation. It was reported as an epileptic aura 22 times out of 271 cases of temporal lobe epilepsy and was produced by stimulation 9 times.

All of these are interpretive responses. They correspond with the judgments which a normal individual is making constantly as he compares present experience with past experience. If a decision is to be made as to whether present experience is familiar or appropriate or menacing, the record of past experience must be available and the new record must be somehow classified with similar old records for the purposes of comparison.





These psychical responses were produced by stimulation of the temporal cortex, chiefly on the superior and lateral surfaces of both lobes and probably extending a little way into the parietal lobe (Fig. 8). None resulted from stimulation of other lobes. It seems fair to conclude, therefore, that these areas of cortex have a particular relationship to the formation of a record of experience and the preservation of that record. If this conclusion is correct, another important advance has been made in the evolution of our knowledge of cerebral localization.

### *Doubling of Conscious Experience*

When stimulation produced an experiential response during operative exploration, the patient usually recognized that this was something out of his own past. At the same time he may have been acutely aware of the fact that he was lying upon the operating table. Thus he was able to contemplate and to talk about this doubling of awareness and to recognize it as a strange paradox.

A young man (J. T., see ref. 3, p. 136) who had recently come from his home in South Africa cried out when the superior surface of his right temporal lobe was being stimulated: "Yes, Doctor, yes, Doctor! Now I hear people laughing—my friends—in South Africa." After stimulation was over he could discuss that double awareness and express his astonishment, for it had seemed to him that he was with his cousins at their home where he and the two young ladies were laughing together. He did not remember what they were laughing at. Doubtless he would have discovered that also, if the strip of experience had happened to begin earlier, or if the surgeon had continued the stimulation a little longer.

This was an experience from his earlier life. It had faded from his recollective memory, but the ganglionic pattern which must have been formed during that experience was still intact and available to the stimulating electrode. It was at least as clear to him as it would have been had he closed his eyes and ears 30 seconds after the event and rehearsed the whole scene "from memory". Sight and sound and personal interpretation, all were re-created for him.

It is significant, however, that during the re-creation of that past experience he was not impelled to speak to his cousins. Instead he spoke to the "Doctor" in the operating room. Herein may lie an important distinction between this form of hallucination and the hallucinations of a patient during a toxic delirium or a psychotic state. In my experience (and relying only on my own memory!) no patient has ever addressed

himself to a person who was part of a past experience, unless perhaps it was when he had passed into a state of automatism.\*

As J. T. lay on the operating table two sets of ganglionic recordings were available to him for his conscious consideration, one that had been laid down during an interval of time that belonged to the past, and another that was being laid down during an equal interval of time in what we may call the present. He was evidently able to distinguish between the present experience and the past and so he addressed himself in astonishment to one of the actors in the present experience.

In the recording which he was then making of the present experience, he was including the experience that came to him from the past, together with the sensory information of his present environment in the operating room, and the results of his reasoning in regard to the two recordings.

When such states occurred in an epileptic attack, Hughlings Jackson spoke of a doubling of consciousness. But there is an important difference in the two experiences. Although the sensory elements may be as realistic in one as in the other, the interpretation in the flash-back was all finished while the interpretation of both experiences had to be made and recorded as a part of the present experience!

When we discussed the matter, the patient and I, during the period of convalescence which followed removal of a large portion of his right temporal lobe, he recalled the whole affair and also his own surprise that he should hear his friends so far away and laugh with them while he faced such a serious situation here in Montreal.

One might suggest that, while the right temporal lobe under the influence of stimulation was engaged in the reproduction of the experience from the past, the left temporal lobe was being employed by the patient in the formation of the recording of the whole present experience. Such a suggestion is, of course, no more than a surmise. But that he did make a new record of both experiences somewhere is certain.

There are two elements in the experiential record, first, the sensory material of which the subject was originally aware and, second, the interpretation of the sensory material with a conclusion as to its significance. As already pointed out, in order to make the second, or interpretive, element possible, there must be comparison with past experience so that a conclusion may be drawn as to familiarity, strangeness, distance, danger,

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\* During automatism patients sometimes talk about unrelated matters, which might suggest that they were addressing someone, but they never describe hallucinations and there is complete subsequent amnesia.

advantage, and necessity for action. It seems likely that under normal conditions the actual recording of the stream of consciousness may be utilized for the purposes of comparison long after it has been lost to voluntary recall.

### *Tempo of Action*

I conclude that the interval of time involved in the past experience is the same as the time required for its subsequent re-enactment. The action or thought in the re-enactment progresses at the same speed as during the original experience. I make this conclusion about speed from consideration of the following evidence:

The patient N. C., whose case has been described, listened to an orchestra while the electrode was applied. When she hummed the air, accompanying thus the music, the tempo of her humming was the tempo that would be expected of an orchestra.

Let me give another similar example.

D. F. (ref. 3, p. 128) was an intelligent young woman, a secretary and amateur musician. After the anterior end of her right temporal lobe had been amputated, the cut surface of the gray matter was stimulated at a point on the superior surface of the lobe. The stimulus caused her to say that she heard an orchestra playing and she asserted that we had turned on a phonograph. When she hummed the tune, Miss Phoebe Stanley, the operating nurse, recognizing the song, supplied the words of the lyric. The tempo of the patient's humming was certainly the tempo that would be expected of an orchestra playing that air.

And so, since the music is reproduced at a normal tempo regardless of the number of electrical impulses per second which may be varied from 30 to 100, I would conclude that the rate of movement in the re-created experience is the same as that of the original occurrence.

Further, and more important, verification of this conclusion is to be found in the fact that no patient has suggested that the people who walked or spoke or called during the hallucination did so at an unusual or unexpected rate of speed.

### *The Patient's Interpretation of an Experiential Response*

Some patients call the response a dream. Others state that it is a flash-back from their own life history. All agree that it is more vivid than anything that they could recall voluntarily.

G. F. (ref. 3, p. 137) was caused to hear her small son, Frank, speaking in the yard outside her own kitchen, and she heard the "neighbourhood

sounds" as well. Ten days after operation she was asked if this was a memory. "Oh, no," she replied. "It seemed more real than that." Then she added: "Of course, I have heard Frankie like that many, many times, thousands of times."

This response to stimulation was a single experience. Her memory of such occasions was a generalization. Without the aid of the electrode, she could not recall any one of the specific instances nor hear the honking of automobiles that might mean danger to Frankie, or cries of other children or the barking of dogs that would have made up the "neighbourhood sounds" on each occasion.

The patients have never looked upon an experiential response as a remembering. Instead of that it is a hearing—and seeing—again, a living-through moments of past time. Do you remember Dickens' Christmas Carol, and how Old Scrooge seemed to re-live certain boyhood experiences under the strange spell of the "Spirit of Christmas Past?" It seems to be a little like that.

D. F. listened to an orchestra in the operating room but did not recall where she had heard it "that way". It was a song she had never learned to sing or play. Perhaps she had been oblivious of her surroundings while she listened to the orchestra in that previous period of time. T. S. heard music and seemed to be in the theatre where he had heard it. A. Br. heard the singing of a Christmas song in her church at home in Holland. She seemed to be there in the church and was moved again by the beauty of the occasion just as she had been on that Christmas Eve some years before.

### *Content of the Record*

The nature of the contents of the record of the stream of consciousness may be guessed from the words of the patients that I have quoted tonight and of patients included in previous publications. It may be surmised also, by any clinician, from critical study of the content of the temporal lobe seizures which Hughlings Jackson called dreamy states. It may be guessed from the fact that when you meet a friend after many years you detect the little changes in him in a way that proves you had not lost the detail of original experiences. It may well be that seeing him renders details of the original record available for comparison, details which were lost to voluntary recollection.

The recording has strong visual and auditory components but always it is an unfolding of sight and sound and also, though rarely, of sense of position. The experience goes forward. There are no still pictures.

Curiously enough, no patient has yet reported pain or taste or smell during an experiential response. These sensations, without recollection of previous experience, were elicited by the electrode only from sensory areas. They were considered by the patient to be no more than present sensations, not elements in a past experience. It should be said, however, that the failure to get a response of any particular type has little statistical value, for the total number of patients from whom psychical responses have been elicited is, after all, small.\*

One might seek to discover whether reasoning, which is divorced from awareness of sensory phenomena, finds any place in the cortical record. It is difficult for me to explore this possibility which involves certain questions of philosophical analysis. But it may be pointed out that patients do sometimes speak of unexpected thoughts coming into mind as a warning of the onset of a focal seizure. They usually report that this confuses them so that the account they give of the matter is not clear.

An example was presented above (Case A. D.) of the production of two thoughts by temporal stimulation. The patient said that one thought had to do with what was going on at the present time and the second thought was different, but the effect upon him was confusion and inability to explain. It might seem that two lines of reasoning or thinking could not co-exist without interference and that, if thoughts were really re-activated, they confused the patient's present effort to rationalize.

However that may be, it seems clear that the final interpretation and the understanding of any experience are recorded with the experience. This interpretation and understanding may be considered the end result or the conclusion of rationalization. Certainly, at the times of re-activation, the patient has no difficulty in perceiving his former understanding of a situation along with the objective aspects of the situation itself.

### *Memory Contrasted With the Record*

It is clear that each successive recording is somehow classified and compared with previous recordings so that, little by little, each separate song is "learned" and becomes a unit in the memory, and all the familiar

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\* My associate Dr. Sean Mullan informs me that there have been 87 cases of temporal lobe epilepsy in which electrical exploration was carried out during the past three years. In only 22 of them did stimulation produce echoes of past experience. We have explored the cortex in 271 temporal lobe cases in all, which suggests that not over 60 patients had experiential responses. In no case where the epileptogenic focus was located in central or frontal regions have there been such responses.

things in a man's life undergo the same change. A poem or an elocution may be "committed to memory". But memory, as we ordinarily think of it, is something more, and a great deal less, than any recording, unless that recording was made unusually vivid by fear or joy or special meaning. Then perhaps the detail of an original experience and the patient's memory of it might be identical.

The psychical responses of the "flash-back" variety were, for the most part, quite unimportant moments in the patient's life: standing on a street corner, hearing a mother call her child, taking a part in a conversation, listening to a little boy as he played in the yard. If these unimportant minutes of time were preserved in the ganglionic recordings of these patients, why should it be thought that any experience in the stream of consciousness drops out?

The evidence suggests that nothing is lost, that the record of each man's experience is complete. The time taken up by deep sleep or coma must drop out and it must be left an open question as to whether or not the time taken up by reasoning is included in the record.

#### CONCLUSION

In conclusion it is evident that the brain of every man contains an unchanging ganglionic record of successive experience. The psychical responses which have been produced by electrical stimulation, during craniotomy and cortical exploration, demonstrate that this record embraces and retains the elements that once were incorporated in his stream of thought.

Simply expressed, the conditions which bring about these psychical responses, both experiential and interpretive, are these: The stimulating electrode, delivering for example 60 impulses per second, is applied to a point on the temporal cortex of a man who is fully awake. The ganglion cells of the cortex are hyper-irritable and ready to react because, for years, small electrical discharges have been playing over the cortical blanket day and night from a neighboring epileptogenic focus.

Thousands of these conditioned ganglion cells may well be reached directly by the stimulating current and they have neuronal connections that pass through the gray matter that covers the temporal lobe and also inward to the central integrating circuits of the brain stem. But instead of mass activity, a selective and highly patterned ganglionic action results.

Let me describe what seems to happen by means of a parable: Among the millions and millions of nerve cells that clothe certain parts of the



temporal lobe on each side, there runs a thread. It is the thread of time, the thread that has run through each succeeding wakeful hour of the individual's past life. Think of this thread, if you like, as a pathway through an unending sequence of nerve cells, nerve fibers and synapses. It is a pathway which can be followed again because of the continuing facilitation that has been created in the cell contacts.

When, by chance, the neurosurgeon's electrode activates some portion of that thread, there is a response as though that thread were a wire recorder, or a strip of cinematographic film, on which are registered all those things of which the individual was once aware, the things he selected for his attention in that interval of time. Absent from it are the sensory impulses he ignored, the talk he did not heed.

Time's strip of film runs forward, never backward, even when resurrected from the past. It seems to proceed again at time's own unchanged pace. It would seem, once one section of the strip has come alive, that a functional all-or-nothing principle steps in so as to protect the other portions of the film from activation by the electric current. As long as the electrode is held in place, the experience of a former day goes forward. There is no holding it still, no turning it back. When the electrode is withdrawn it stops as suddenly as it began.\*

We have found a way of activating the anatomical record of the stream of consciousness. It is evident, therefore, that the ganglionic mechanism which preserves man's experiential record is either present, in duplicate, in the temporal cortex of each hemisphere, where stimulation produces these responses; or it is located in duplicate in the hippocampal zones of each side where direct stimulation does not produce the responses; or, finally, it is located more centrally in the brain where the closest functional connection is maintained with the stimutable zones of the temporal lobes.

However that may be, and whatever the mechanism involved, it is certain that in the temporal cortex lie the keys of activation of the record.

During any given period of waking time each individual forms a record of the stream of consciousness. The record is the final expression, and the outcome, of the action of central integration of nerve impulses. The

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\* A particular strip can sometimes be repeated by interrupting the stimulation and then reapplying it at the same or a nearby point, for the threshold of evocation of that particular response is lowered for a time by the first stimulus. Graham Brown and Sherrington (6) described local facilitation and intensification of motor responses by repeated stimulation at a single point in the anthropoid cortex, and we have found the same to be true for man in motor and sensory areas of the cortex (2).



formation of this record is subject to the selecting and limiting influences of attention. As the record is formed, it includes the elements of consciousness. Possibly, like a film, its contents are projected on the screen of man's awareness before it is replaced by subsequent experience. Thus it might seem that the record of the stream of consciousness is more than a record. It represents one of the final stages in the neuronal integration which makes consciousness what it is.

Probably no man can, by voluntary effort, completely re-activate any portion of the record of the stream of thought. Except for a few seconds or minutes after the event, he seems to have no voluntary mechanism that rivals the electrode. Memory, as ordinarily conceived, is quite a different phenomenon. It seems likely, however, that the original record continues to be available in some sort of way for the purposes of the comparison and interpretation of each new experience, as long as a man may live and keep his wits.

The stream of consciousness flows inexorably onward, as described in the words of William James. But, unlike a river, it leaves behind it a permanent record that seems to be complete for the waking moments of a man's life, a record that runs, no doubt, like a thread along a pathway of ganglionic and synaptic facilitations in the brain. This pathway is located partly or wholly in the temporal lobes.

There is hope in all this that physiology and psychology, and philosophy too, may be drawn more closely together and that, with the opening of a new chapter of understanding of the localization of function within the human brain, some light may yet be thrown upon the mind of man.

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THURSDAY JUNE 10 — JEUDI LE 10 JUIN

*Chairman — Président*

MARIO PONZO

*Ordinario di psicologia nell'Università di Roma*

## PERCEPTION ET COGNITION

PAR

BARON ALBERT MICHOTTE VAN DEN BERCK

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*(Le Professeur Michotte a été présenté par Sir Frederic C. Bartlett,  
Professor of Psychology at Cambridge University)*

Note : A la demande de nombreux collègues d'Amérique, cette conférence a été donnée en anglais, au Congrès. Elle a été suivie d'un certain nombre de démonstrations. Le Professeur Michotte a voulu que cette conférence soit publiée ici en français.

In answer to the request of several American colleagues, this address was given in English at the Congress. It has been followed by a certain number of demonstrations. Professor Michotte has asked that this conference be published here in French.

Il y a vingt-cinq ans, j'avais l'honneur, en des circonstances pareilles de m'adresser un soir, aux membres du IXème Congrès International de Psychologie tenu à Yale en 1929. Le sujet de ma conférence était alors : « Perception and Action », et j'avais tenté de mettre en lumière les principaux problèmes qui, dans mon opinion, se posaient à propos des rapports existant entre ces deux aspects du comportement.

Depuis lors, je me suis efforcé, avec mes collaborateurs, de répondre à certaines des questions que j'avais formulées à cette occasion ; nous avons étudié entre autres, la perception de la causalité, la réalité apparente du monde physique, la permanence phénoménale, etc. (1)

Et comme il arrive toujours en semblables circonstances, les expériences entreprises pour résoudre un problème en font apercevoir d'autres. Et c'est

ainsi que l'examen des conduites des sujets au cours de toutes ces recherches a attiré notre attention sur des particularités qui me paraissent intéressantes, parce qu'elles touchent à certains aspects théoriques du vaste domaine des relations entre les différents processus cognitifs.

Afin de préciser l'objet de la présente discussion, je voudrais prendre comme point de départ une distinction de simple bon sens ; celle entre trois moyens qui nous permettent de recueillir des renseignements au sujet du monde physique (notre corps y compris).

Le premier est l'événement perceptif concret, particulier, qui se produit à un moment de notre vie sous l'influence d'un système d'excitations agissant sur nos organes des sens, et qui se montre directement lié audit système. Cet événement qui présente du reste de nombreux aspects (sensoriel, intellectuel, affectif . . .) nous fait connaître un objet, un processus, ou un groupe d'objets ou de processus, ou encore une situation spatio-temporelle complexe, et constitue la base de toutes les connaissances que nous acquérons au sujet du monde physique. Conformément à l'usage le plus répandu mais nullement exclusif, malheureusement à notre avis, c'est ce sens du mot perception que nous adopterons dans la suite.

Est-il besoin de dire que ce sont des événements de cette sorte que l'on tente de produire dans un état d'isolement relatif, dans les conditions artificielles des laboratoires, et que ce sont eux dont s'occupent en général les travaux sur « la perception », qu'il s'agisse d'expériences de psychophysique, ou de recherches plus compliquées portant sur l'influence de l'expérience passée, des attitudes, de la motivation, de la personnalité, etc.

Mais dans la vie courante, la perception n'est pas isolée (ce n'est du reste que par une fiction qu'on la considère comme telle dans les laboratoires) car nos organes des sens sont constamment sollicités par un flot d'excitations qui sont en évolution permanente, soit par suite des changements qui se produisent dans le monde extérieur, soit comme conséquence de nos actions et des mouvements de notre corps. Ces excitations nous fournissent un nombre incalculable de données perceptives qui, envisagées en elles-mêmes, ne constituent qu'une série d'événements éphémères successifs, mais qui, de fait, nous apportent des matériaux que *synthétise sans arrêt notre activité propre*, créant et revisant continuellement la façon dont nous concevons le monde et les lois qui le régissent, sur la base de notre expérience personnelle. Quelle que soit l'opinion que l'on professe quant à la nature de la pensée, il est évident que l'élaboration d'innombrables données perceptives, leur comparaison, leurs liaisons, leur analyse, l'abstraction, la généralisation, les inférences, etc. élargissent prodigieusement nos connaissances ; elles nous permettent d'atteindre un niveau

qui dépasse la connaissance perceptive directe, dans la mesure où la compréhension et l'explication dépassent une simple constatation.

Seulement ce travail est, dans les conditions sociales dans lesquelles nous vivons, singulièrement aidé par le troisième moyen d'information dont nous disposons : la *communication verbale* entre différents individus. Celle-ci nous permet d'intégrer dans notre savoir les résultats des expériences ou de la réflexion d'autrui. La communication verbale nous fournit (à partir de la perception du langage, que ce soit la parole des parents, du maître ou de toute autre personne, la lecture d'une revue ou d'un journal), des données dont l'influence sera sans doute variable d'après les sources, mais qui, en tout état de cause, et malgré le caractère symbolique de leur transmission, s'intègre aux acquisitions faites sur la base de l'expérience personnelle, et les enrichit de fait dans une proportion incalculable.

La plupart du temps, au cours de notre vie, les renseignements provenant de la perception actuelle, les connaissances précédemment acquises qui s'y rapportent, et ce que d'autres personnes nous apprennent à leur sujet, sont concordants.

Il ne manque pas de cas, cependant, dans lesquels certaines de ces informations sont contradictoires, et ceux-ci peuvent être fort instructifs. La contradiction peut naturellement se manifester de toutes manières possibles. Ce peuvent être les rapports verbaux de divers individus qui s'opposent les uns aux autres ou bien, telle expérience présente peut-être inconciliable avec une constatation ou un apprentissage antérieurs, etc. (2). Nous nous bornerons à l'examen de cas dans lesquels sont impliqués d'une manière ou de l'autre les données perceptives actuelles car c'est principalement à propos de ceux-là que nous avons pu réunir un certain nombre de documents.

Le premier groupe de cas qui doit nous occuper est celui dans lequel l'opposition est intrinsèque au processus perceptif lui-même. On peut le réaliser en combinant un ensemble complexe de stimulations, dont certains systèmes partiels sont de nature à provoquer la formation de structures perceptives présentant entre elles des incompatibilités plus ou moins accusées. Nous appellerons dans la suite « stimulations discordantes » des situations de cette nature (3). Celles-ci ont des conséquences fort différentes d'après les circonstances, et particulièrement sur les configurations perceptives elles-mêmes.

C'est notamment à de pareilles discordances qu'il faut attribuer souvent sans doute, ces cas curieux, assez fréquents au laboratoire, dans lesquels les observateurs disent : « Qu'ils ne savent pas ce qu'ils voient » (!) Cette expression ne signifie d'ailleurs pas seulement que « ce-qu'ils-voient » ne

correspond à aucune classe de concepts nettement définis, et moins encore que le vocabulaire dont ils disposent ne comprend pas de termes adéquats pour l'exprimer. C'est l'impression elle-même qui est confuse, chaotique. On fait apparaître facilement ce phénomène en montrant aux sujets plusieurs objets exécutant des mouvements plus ou moins irréguliers, ou même un seul objet dont les modifications qu'il subit sont complexes, changements de forme alliés à des mouvements de pivotement, etc. Les observateurs « savent » évidemment qu'ils voient des mouvements, mais ceux-ci sont désordonnés et leur ensemble est totalement dépourvu d'organisation.

A l'extrême opposé, il y a des cas dans lesquels le même objet apparaît simultanément sous des formes différentes, inconciliables. En voici un exemple. Au cours d'expériences sur la perception des volumes, nous présentions aux sujets des hémisphères de bois de 7 à 10 centimètres de diamètre environ, dont la face postérieure était plane. Ces hémisphères, vus de face, en vision binoculaire aussi bien que monoculaire, donnent une impression de sphère complète ; on voit « une boule », phénomène singulier puisqu'il est impossible de percevoir à la fois les faces antérieure et postérieure du solide. Il y a là un problème fort intéressant qui est encore à l'étude, mais il semble bien que tout cela soit en rapport avec ce que nous avons appelé l'aspect « amodal » de l'expérience (4). Quoi qu'il en soit de ce point, un fait étrange a été observé par le Professeur Buytendijk lors d'une de ses visites à Louvain. Il a remarqué que si le sujet tâta la face postérieure de l'hémisphère, pendant qu'il le regardait, il pouvait parfaitement continuer à la voir comme une sphère, tout en sentant, par voie tactile-kinesthésique que l'envers était plat ! Et la contradiction ne suscite aucun trouble, aucune gêne ! L'expérience a été répétée bien souvent depuis lors et le même résultat a été obtenu sans aucune difficulté.

Il y a donc ici coexistence de configurations contradictoires, et l'on se demande comment pareille chose est possible. On pourrait supposer, par exemple, que l'unité d'objet n'existerait que sur le plan intellectuel, au niveau duquel la contradiction serait éliminée par l'introduction du concept d'illusion, sur lequel nous reviendrons plus tard. Il y a cela évidemment, car le sujet est certain de la planéité de la face postérieure, et ne *croit* plus à la sphéricité qu'il « voit » ; et cette certitude se manifeste en ce que la manipulation de l'objet se fait en tenant compte de la planéité. Pourquoi en va-t-il ainsi ? Une solution de facilité consisterait à dire que les impressions tactiles-kinesthésiques sont « dominantes » par rapport aux visuelles. Mais l'insuffisance d'une telle explication saute aux yeux lorsqu'on songe

que l'on peut trouver aisément quantité d'exemples dans lesquels c'est au contraire l'impression tactile-kinesthésique qui est considérée comme illusoire tandis que l'on a « foi » dans les données visuelles. La question est plus compliquée et il n'est pas impossible que, dans le cas de l'hémisphère, l'aspect « amodal » de la face postérieure de la sphère favorise le caractère d'illusion qu'on lui attribue.

Malheureusement, la notion d'illusion est loin de résoudre toute la question, car l'identité de l'objet est évidente sur le plan phénoménal aussi. C'est la même sphère « vue », qui est « sentie » comme objet à fond plat ! Et chose curieuse, il n'y a aucune manifestation d'inhibition. Ceci touche évidemment au problème des relations entre les impressions de forme notamment, des diverses modalités sensorielles, et paraît indiquer que, dans certaines conditions et malgré l'identité de la chose perçue, tout se passe comme si l'on avait affaire à deux mondes différents, sans intercommunications. Ceci rejoint toutes les observations qui tendent à prouver l'autonomie originelle des différents domaines sensoriels. A ce point de vue, des expériences systématiques d'apprentissage portant sur l'influence d'un exercice moteur sur les structures visuelles, un peu dans le genre de celles dont il sera question ultérieurement, mais adaptées spécialement au problème de la perception des formes, apporteraient peut-être quelques précisions sur toute cette question.

Entre les deux cas extrêmes que nous venons de décrire, brouillage et coexistence, s'en situent une quantité d'autres qui ont ce trait commun que la configuration perceptive est unitaire et se présente comme un tout bien organisé, mais que celui-ci est influencé d'une manière ou de l'autre par les deux systèmes de stimulations discordants, qu'il arrive à concilier. En d'autres mots, il s'agit d'un « compromis » ; mais si le nom est unique, les réalisations sont multiples, et tellement variées tant au point de vue de leur nature qu'à celui de leur mode d'apparition (immédiat, après une série de répétitions, après un apprentissage systématique), etc. qu'il est bien difficile de les classer. Nous ne chercherons pas à le faire, nous nous contenterons de donner quelques points de repère.

Un premier exemple est celui de la perception des dessins perspectifs. Il y a longtemps que les psychologues ont signalé que l'on « voit » la troisième dimension lorsqu'on regarde de pareilles figures. Mais ceci ne laisse pas d'être assez troublant. Aussi avons-nous repris la question, en procédant à des expériences au cours desquelles nous demandions aux sujets de réaliser une égalité apparente de profondeur entre des modèles qui leur étaient présentés dans des conditions déterminées, et un parallépipède dont les arêtes étaient faites de fines tiges métalliques, et dont on

pouvait varier à volonté la profondeur par un mécanisme approprié, la section étant maintenue constante. Quant aux modèles ils étaient soit d'autres parallélépipèdes réels, de même construction, mais de grandeurs fixes, soit des images représentant une vue perspective géométriquement exacte de ces modèles, du point de vue des sujets. Or il est apparu que ceux-ci procédaient aux « ajustements » avec autant de facilité, ou fort peu s'en faut, pour les modèles réels et pour les dessins. Et pour ce qui est des résultats objectifs, la profondeur apparente est, en général (pas dans toutes les conditions) réduite dans le deuxième cas, et la précision des mesures est du même ordre. Il est donc certain que ce n'est pas l'absence de profondeur phénoménale de l'objet représenté, qui caractérise le dessin perspectif.

On y voit un objet vraiment tri-dimensionnel, mais le volume perçu manque de réalité, le caractère de réalité devant être considéré ainsi que nous croyons l'avoir démontré, comme une « dimension » particulière de l'expérience perceptive, susceptible de variations quantitatives comme les autres « dimensions » de cette expérience. Et c'est précisément en cette perte de réalité, s'alliant au maintien de la tri-dimensionnalité, que se manifeste le compromis entre les exigences contradictoires de la stimulation (5).

Il est facile du reste, de montrer que l'on a bien affaire en l'occurrence à un système de stimulations discordantes. En effet, il suffit de libérer l'ensemble des lignes constituant le dessin perceptif, du support plan sur lequel elles sont dessinées, pour qu'elles apparaissent comme un volume *réel*. Ainsi, on peut au moyen de fines tiges métalliques reproduire exactement la forme du dessin perspectif plan, et cette figure, se détachant sur un fond distant, donne une impression nette de volume réel en vision monoculaire (et même binoculaire, lorsque la distance est suffisante pour éviter la perception de la planéité de l'objet). Cette impression est encore plus marquée lorsqu'on regarde, même de très près, une figure métallique de ce genre en vision monoculaire et d'un point de vue déterminé ; et le caractère de réalité du volume est alors absolument convaincant, aussi évident, aussi palpable que dans le cas d'un solide perçu en vision binoculaire (6).

Par contre, dans le cas du dessin, les facteurs d'organisation structurale de la perception tendent à faire voir les lignes comme des « traces », adhérent et appartenant à un support plan.

Il y a donc contradiction entre la tendance des lignes à donner l'impression d'un volume réel, et le confinement de l'ensemble dans un plan. Et le compromis apporte une solution satisfaisante, car il est clair que du



moment où il ne s'agit plus d'un volume réel, mais d'une simple apparence, celle-ci peut se concilier avec le caractère de planéité du support (7).

Peut-être n'est-il pas inutile de souligner que dans un cas comme celui-ci, nous savons qu'il y a « illusion », puisque nous ne croyons pas nous trouver en présence d'un solide réel, mais, à la différence de beaucoup d'illusions dont nous devons apprendre par expérience le caractère trompeur, il y a davantage ici, puisque le caractère d'irréalité du volume nous apparaît phénoménalement dans la configuration perçue elle-même.

*On pourrait considérer en quelque sorte, me semble-t-il, cette absence de réalité apparente comme le parallèle, le substitut, sur le plan phénoménal, de la notion d'illusion dans le domaine intellectuel. L'un et l'autre vont souvent de pair, mais il y a non moins souvent dissociation entre elles, et c'est ce qui arrive dans tous les cas où l'on a affaire à des illusions connues comme telles, mais où le contenu perceptif apparaît comme parfaitement réel (illusions de Müller-Lyer p. ex., etc.).*

Tout le monde connaît les systèmes de stimulations ambiguës qui se rencontrent fréquemment aussi bien dans la vie courante qu'au laboratoire, et qui donnent des impressions diverses à différentes personnes, ou se manifestent par des changements dans les configurations perçues par un individu lors d'observations prolongées, ou même simplement répétées.

L'exemple le plus courant est celui des images-devinettes que l'on trouve partout ; puis, il y a les perspectives réversibles, l'inversion apparente des mouvements, le « moulin de Sinsteden » etc. Ces phénomènes répondent naturellement à des différences ou à des modifications de l'état de réceptivité des sujets, sur la nature desquelles on est du reste encore loin d'être d'accord. Mais ce qui nous intéresse ici, c'est que les stimulations discordantes, dont nous nous occupons peuvent également intervenir pour provoquer des variations dans les impressions, et déterminent souvent une évolution vers des formes de plus en plus stables.

Une excellente démonstration peut en être fournie par des essais qui se rattachent à une ancienne expérience de Benussi sur la transparence apparente (8). Celle-ci consistait à faire voir aux sujets un disque tournant assez lentement, autour de son axe, et vers la périphérie duquel étaient dessinées deux circonférences chevauchant partiellement l'une sur l'autre. Dans ces conditions les lignes tracées délimitent, objectivement, trois surfaces adjacentes : deux croissantes 1 et 3, encadrant un fuseau 2 (voir Fig. 1). Conformément aux lois de la perception des formes, les observateurs voient néanmoins sur le disque au repos deux circonférences complètes. Et lorsque le disque est mis en mouvement et que l'on suit des yeux la figure, la configuration qui s'établit de façon prédominante est celle



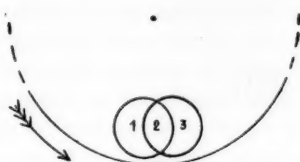


Fig. 1.

de deux circonférences qui exécutent des mouvements de rotation excentrique en glissant l'un sur l'autre.

Si l'on supprime alors les lignes de la figure, et que l'on colorie simplement chacune des trois plages de façon qu'elles aient des teintes différentes : 1 et 3 rouge et jaune par exemple, et 2 orange, le système de stimulations devient déjà plus discordant. En effet, étant donné la continuité des contours, il y aura évidemment de nouveau tendance à voir deux cercles complets, mais, d'autre part, les différences de couleurs sont de nature à ségréger les trois plages, et le fuseau central interrompt la continuité géométrique en produisant une échancrure dans chaque cercle. Or, comme l'a montré Benussi, il s'établit dans ces conditions une structure nouvelle, celle de la transparence. On voit en général un cercle de teinte uniforme, mais transparente tourner partiellement devant un autre cercle de teinte uniforme lui aussi, que l'on voit, à travers le premier au niveau de leur recouvrement. L'impression est naturellement d'autant plus parfaite et d'autant plus aisée à produire que les différences de teintes sont mieux appropriées à cette structure. Ce n'est cependant pas la seule configuration qui se présente, loin de là, et il arrive que d'autres la précèdent chez l'observateur avant que celle-ci s'établisse, mais une fois ce résultat obtenu il semble fort stable. Et, chose remarquable, cette configuration concilie les oppositions, car, grâce à la transparence, il n'y a évidemment plus de contradiction entre la forme des cercles et les couleurs, puisque les unes et les autres sont apparemment continues et uniformes pour chacun d'eux.

Ce genre d'expériences peut être varié à l'infini, mais nous voudrions citer ici une modification intéressante qu'elle peut subir. Celle-ci consiste simplement à accentuer encore la discordance en utilisant d'autres couleurs, par exemple bleu et rouge pour les deux croissants et un jaune vif pour le fuseau. Dans ces conditions et d'après le dessin de la figure, ses dimensions, les teintes employées, etc. on peut obtenir des résultats extrêmement différents dont voici, à titre d'exemple un échantillon emprunté à des observations que j'ai faites personnellement. J'ai vu d'abord deux disques opaques glissant l'un par rapport à l'autre. Le disque

inférieur était rouge et de teinte uniforme sur toute sa surface, tandis que le disque qui le recouvrait partiellement était bicolore (fuseau jaune et croissant bleu). Après quelques moments d'observation, la structure s'est modifiée dans le sens de la transparence, le disque le plus rapproché m'a semblé transparent ; je voyais le disque d'arrière (le rouge) à travers le disque bleu qui le recouvrait, mais au fur et à mesure du déplacement de ce dernier, la partie recouverte du disque rouge changeait de couleur et devenait jaune. Cette configuration présentait sur la précédente l'avantage de l'uniformité phénoménale de la teinte de chaque disque ; mais ceci d'autre part n'a été obtenu que grâce à ce singulier changement de couleur se produisant parallèlement au mouvement du disque supérieur, et qui ne correspondait, en aucune façon, bien entendu, ni aux lois psychologiques, ni aux lois physiques des couleurs.

Continuant encore l'observation j'ai vu soudain apparaître une tout autre forme, un cylindre creux dont la surface extérieure était bleue, l'intérieur rouge, et le fond jaune ; le tout exécutant un mouvement compliqué de rotation et de bascule. Cette observation a été confirmée par de nombreux sujets, et il n'est pas difficile de fixer les conditions dans lesquelles la configuration du cylindre se produit le plus rapidement. Ajoutons qu'une fois établie, elle se montre de nouveau fort stable. Ici, grâce à l'extension de l'objet dans la troisième dimension, à l'intervention de la perspective et de l'effet écran, les anomalies dans la répartition des couleurs sont réduites à un minimum, car les parois extérieure et intérieure du cylindre ont des teintes uniformes, et il en va de même du fond jaune qui semble être un cercle complet, dont les différentes parties sont rendues successivement visibles au cours de la rotation.

Nous retrouvons dans cette dernière configuration, la manifestation de la discordance dans la stimulation, mais elle est éliminée d'une façon remarquable par l'intervention de la troisième dimension sous forme perspective.

Il arrive parfois cependant que les choses se compliquent. J'ai observé notamment qu'il était possible de voir le fond circulaire du cylindre adhérer complètement au plan du disque, ce qui allait de pair alors avec un mouvement invraisemblable de contorsion du corps du cylindre par rapport à sa base !

Une autre modification, très instructive de l'expérience des cercles excentriques, consiste à remplacer l'un d'eux par un carré dont le sommet de l'un des angles coïncide avec le centre du cercle (Fig. 2), que doit fixer l'observateur.

Pendant la rotation du disque on peut aisément voir le carré pivoter et



Fig. 2.

faire un tour complet autour de ce centre, recouvrant successivement toutes les portions de la circonférence. Ceci est déjà assez curieux, et provient, comme il est aisé de le démontrer de ce que, au cours de la rotation du disque, le carré change d'orientation, tandis que le cercle, grâce à sa forme dont est absent tout point de repère, paraît maintenir continuellement la sienne. Mais ce n'est pas là le point sur lequel je voudrais insister ici ; c'est plutôt sur le fait que lorsque les figures sont dessinées assez près du bord extérieur du disque de façon qu'il soit impossible pour le carré d'effectuer physiquement une rotation autour du sommet de l'un de ses angles sans déborder la limite du disque, ceci n'empêche nullement la perception de ladite rotation ! Le carré continue à faire apparemment un tour complet !

Le comportement des sujets est assez curieux à noter en présence de cette expérience. Il y en a un certain nombre qui enregistrent simplement la rotation sans remarquer même qu'il n'y a pas assez de place pour que le carré puisse tourner. D'autres aperçoivent l'anomalie et tâchent de rationaliser leur expérience en imaginant que les choses se passeraient de la même manière s'il y avait rotation physique du carré, par suite de la combinaison de ce mouvement avec la révolution du disque ! Et enfin, il y a ceux qui comprennent qu'il est impossible de faire réellement tourner le carré de cette façon ; cette constatation n'altère d'ailleurs en aucune manière leur impression de mouvement comme tel, ils la considèrent simplement comme une singularité qu'ils ne s'expliquent pas d'emblée, bien entendu, et se satisfont intellectuellement en se disant qu'il s'agit encore une fois d'une illusion !

Un nouvel exemple de discordance, dans lequel cette fois le rôle de l'apprentissage est plus marqué, est emprunté à des recherches que nous avons poursuivies récemment à Louvain, avec le Dr. A. Glynn sur la transparence apparente et le phénomène de Rosenbach (9). Lorsqu'on place un écran opaque, soit une bande étroite de papier sur un dessin quelconque, celui par exemple d'un rectangle, de façon que des parties de la figure soient visibles de part et d'autre de la bande, on a l'impression,

comme chacun sait, que la bande recouvre une figure continue, ininterrompue dans la région cachée. C'est une manifestation de l'effet écran. Mais évidemment, on ne voit pas la figure à travers l'écran opaque (dans notre terminologie, elle est phénoménalement présente sous forme « amodale »). Toutefois, Rosenbach a démontré, il y a un demi-siècle déjà, que, dans certaines conditions, on pouvait avoir l'impression que l'écran était vraiment transparent ou tout au moins translucide. Nous avons cherché à reproduire ce phénomène et à déterminer systématiquement les conditions qui influençaient sa manifestation.

Le dispositif expérimental que nous avons adopté se présentait de la manière suivante. Le sujet se trouvait devant un carreau de verre dépoli sur la face antérieure duquel était assujettie une bande étroite et absolument opaque, de teinte grise (l'écran). Le verre dépoli était en outre légèrement éclairé par l'arrière, et l'on avait arrangé les choses de façon à pouvoir projeter sur sa face postérieure une figure, un rectangle en l'espèce, qui, du point de vue du sujet apparaissait gris également mais légèrement plus clair que l'écran. Enfin, on pouvait faire se mouvoir cette figure, qui traversait alors de part en part la surface du verre dépoli, en suivant une trajectoire fortement incurvée et, de plus, exécutait un mouvement de bascule assez bizarre au moment où elle passait derrière l'écran opaque.

Le système d'excitations constituait de nouveau une stimulation discordante, car l'écran opaque cachait une partie du rectangle au moment où celui-ci le croisait, de façon que la structure normale aurait dû être celle de l'effet écran (ou tunnel) ; et c'est celle-ci qui apparaissait en général au début chez tous les observateurs. Mais d'autre part, les facteurs tendant à assurer l'unité de la figure et la continuité de ses contours, étaient beaucoup plus puissants ici que dans le cas de dessins statiques. C'étaient d'une part, le fait de voir le rectangle se mouvoir en entier avant et après le passage au niveau de l'écran ; et, pendant ce passage, le fait que les parties de la figure qui dépassaient de part et d'autre l'écran, exécutaient un mouvement d'ensemble très particulier (loi du sort commun). Ceci ne suffit pas d'ailleurs à expliquer toute l'illusion, comme l'a démontré Glynn.

De fait, lorsqu'on opère dans les conditions optimum et que l'on répète l'expérience un certain nombre de fois, l'effet tunnel est progressivement (parfois même brusquement) remplacé par une impression de translucidité plus ou moins accusée de l'écran, à travers lequel on croit voir la partie du rectangle que le tunnel cache en réalité. La netteté de cette impression et la rapidité avec laquelle elle se développe, varient du reste considérablement d'après les sujets. Pour certains, mais ils sont rares, il faut exercer

une suggestion puissante avec l'aide de présentation d'écrans physiquement translucides. En tout état de cause, le phénomène est, en général, très facile à observer, mais il se développe graduellement.

On pourrait peut-être parler ici aussi d'un « compromis », car si l'écran perd son opacité dans une certaine mesure, la partie du rectangle que l'on croit apercevoir à travers lui est plus sombre que les parties qui sont découvertes. Quoi qu'il en soit, ce qu'il importe de souligner, c'est le changement de structure qui se produit sous l'influence de la stimulation discordante, et le fait que la configuration de translucidité permet aux conditions antagonistes de la perception de manifester simultanément leur effet.

Un dernier cas doit nous retenir, et il paraît tout spécialement intéressant, parce qu'il met mieux encore en lumière le rôle que l'apprentissage peut jouer dans l'établissement des structures perceptives.

La plupart des psychologues connaissaient les fameuses chambres déformées imaginées et construites par Ad. Ames. Un modèle réduit en a été exposé dans les locaux du Congrès, tandis que des originaux réalisés à l'échelle humaine se trouvent à l'Université de Princeton. Il s'agit, on s'en rappellera, de chambres dont les murs, le plafond, le plancher, les fenêtres, au lieu d'être rectangulaires ou carrés, verticaux ou horizontaux comme ils le sont dans des chambres normales, ont des formes trapézoïdales et des inclinaisons plus ou moins accentuées. Celles-ci sont calculées de telle sorte que, étant donné les lois de la perspective, l'image rétinienne formée dans l'œil d'un observateur regardant l'intérieur de la chambre en vision monoculaire, et d'un point de vue déterminé, soit semblable à celle que donnerait une chambre normale dans les conditions de la vie courante. La chambre paraît donc orthogonale, les fenêtres rectangulaires, etc. ; c'est un trompe l'œil quasi parfait. Or Kilpatrick a fait des expériences montrant qu'il était possible, par apprentissage, de faire « voir » aux observateurs la forme « réelle » de la chambre (10).

L'apprentissage lui-même s'est fait de manière différentes. Dans un premier groupe d'expériences, les sujets tentaient de lancer des balles contre un point lumineux que l'on pouvait projeter en divers endroits des murs de la chambre ; ou bien, on leur donnait un long bâton au moyen duquel ils devaient suivre les contours de la chambre, etc.

Dans le cas du second groupe d'expériences, ces opérations étaient réalisées par l'expérimentateur, et le sujet devait simplement observer ce qui se passait dans la chambre.

De fait, malgré des différences individuelles assez marquées, les deux catégories de sujets ont signalé qu'il se produisait un changement dans la

forme apparente de la chambre, et ce changement se produisait dans le sens de la forme « réelle ». Les résultats ont été, en gros, les mêmes pour les deux groupes, avec peut-être une légère supériorité de l'apprentissage actif.

Ces expériences peuvent être rapprochées des précédentes, car ici aussi, il y a discordance dans le système des stimulations mais, de plus, la discordance se manifestait à l'observateur, ce qui n'était guère le cas dans les exemples décrits jusqu'à présent. Du point de vue du sujet en effet, les parois, les angles et les arêtes de la chambre semblent se trouver à des distances (par rapport à lui) correspondant à l'aspect statique, orthogonal qu'elle présente ; et d'autre part, lorsqu'il doit atteindre différents points de ladite chambre, il s'aperçoit que la distance est toute différente. De même quand il est simple témoin, il doit remarquer que l'amplitude des mouvements des balles ou du bâton ne concorde pas avec les distances apparentes.

La modification de forme de la chambre résultant de l'apprentissage a évidemment pour conséquence d'éliminer la contradiction.

Cette expérience est remarquable à divers points de vue lorsqu'on la compare aux précédentes. Signalons d'abord, à titre de simple curiosité, que la solution apportée à la discordance a eu pour effet, ici, de rendre les impressions conformes à la situation physique tandis que dans les autres, elle a eu pour effet de les en éloigner ! Mais en outre, les changements qui se sont manifestés précédemment se faisaient en général dans le sens d'organisations perceptives fréquentes dans la vie courante : transparence, récipiends creux, etc., alors qu'ici la modification se produisait dans une direction opposée à l'expérience journalière que nous avons des chambres à plancher et plafond horizontaux, à angles droits, etc. De plus, il est surprenant de constater avec quelle facilité une structure aussi stable que celle d'une chambre orthogonale normale peut se modifier. Il est possible d'ailleurs, comme l'auteur le signale lui-même, que des imperfections inévitables dans la réalisation des chambres aient agi comme adjuvant (11). Enfin, ces expériences permettent de poser un problème assez curieux. Comment se fait-il que ce soit la structure de la chambre qui se soit modifiée plutôt que l'amplitude apparente des mouvements ? Car après tout, considérant notamment les essais dans lesquels l'observateur devait tenter de suivre les contours de la chambre au moyen d'un bâton, il serait parfaitement concevable que l'apprentissage aboutisse à modifier l'appréciation de l'extension des mouvements exécutés, plutôt que le cadre auquel ils se réfèrent.

Il n'est pas impossible que ceci soit en rapport avec le fait que les sujets

savaient d'avance que l'expérience portait sur des chambres déformées ; et que si l'on refaisait l'expérience d'une manière absolument « unwissentlich » à ce point de vue, ce serait plutôt l'extension des mouvements qui se trouverait influencée par l'apprentissage. Ceci n'est du reste qu'une pure hypothèse, suggérée par la seconde partie de cette conférence.

Il est intéressant de rapprocher les résultats de Kilpatrick de ceux qui ont été obtenus dans les recherches récentes de Kohler sur les effets du port de lunettes produisant soit l'inversion du champ visuel, soit des altérations de forme locales. Dans ce dernier cas, comme on le sait, l'apprentissage a également donné lieu à des modifications de structures visuelles. Il y a là évidemment un immense champ de recherches qui s'ouvre pleinement à présent.

Les cas de stimulations discordantes que nous venons de signaler peuvent être considérés, du point de vue de l'expérimentateur, comme étant des situations problématiques en présence desquelles l'organisme du sujet doit trouver une solution. A vrai dire on pourrait en dire autant de toute perception organisée qui se présente comme réponse à un système plus ou moins compliqué, et par le fait même plus ou moins ambigu, d'excitants. C'est-à-dire, en pratique, dans le cas de toutes les perceptions de la vie courante, et même de tout phénomène d'adaptation biologique.

On a maintes fois souligné également les analogies existant entre les principes qui régissent la formation des structures perceptives et celles de la pensée, et l'intervention des premières dans la solution de problèmes ; mais il semble que les stimulations discordantes constituent un matériel de choix à ces différents points de vue.

Le problème qu'elles recèlent n'est pas formulé sans doute, ni verbalement, ni même mentalement, puisqu'il trouve son origine dans des conditions de stimulations que le sujet ignore ; aussi celui-ci ne se rend-il la plupart du temps nullement compte de ce qu'il existe un « problème ». Il en va autrement de la part de l'expérimentateur qui, lui, a cherché à introduire systématiquement des discordances définies dans le système des stimulations et qui, en faisant agir les excitants sur le sujet provoque forcément chez celui-ci l'apparition d'une réponse perceptive. Telles sont les données du problème ; sa solution est fournie par la nature même de la réponse, ou des réponses successives du sujet.

Lorsqu'on envisage les choses sous cet angle, il est intéressant de comparer les situations de stimulations discordantes à celles de la recherche intentionnelle de solutions de problèmes. Et l'on constate rapidement qu'il ne manque pas d'analogies, tant au point de vue de la nature des solutions qu'à celui de la manière dont elles apparaissent. Ainsi, pour ce qui



concerne le rôle de l'intuition (de l'Insight), il est frappant d'observer combien simples et bien adaptés peuvent être les moyens, souvent tout à fait inattendus, qui apparaissent soudainement et concilient des exigences que l'on pouvait croire incompatibles.

D'autre part, on voit se poser à nouveau l'éternelle question des rôles respectifs qu'il faut assigner au simple jeu des lois fondamentales de la structuration, et à l'expérience acquise en matière de perception de formes. Le nombre incalculable de celles que l'on a vues dans la vie courante, ne mettent-elles pas le sujet en possession de tout un arsenal de solutions potentielles applicables inconsciemment en cas de difficulté, et le rôle de ces formes ne pourrait-il être assimilé, *mutatis mutandis*, à celui que remplit dans d'autres domaines l'application de principes ou de formules connus ?

De même, nous avons constaté que certaines solutions, très satisfaisantes à certains points de vue, présentent des défauts, des anomalies évidentes sous d'autres rapports (par exemple le mouvement du cylindre dans l'expérience des cercles excentriques). La solution adoptée ne fait disparaître une difficulté que pour en créer une autre ! Alors pourquoi celle-ci plutôt que celle-là ? Ceci rappelle étrangement ce que, dans le langage courant on appelle les « solutions boiteuses » !

Enfin, les cas de coexistence de données contradictoires font naturellement songer sur le plan intellectuel à certains sophismes auxquels ils sont si prochainement apparentés que l'on pourrait à bon droit, semble-t-il, parler de « sophismes perceptifs ». Dans un cas comme dans l'autre en effet, il arrive que la solution proposée paraisse s'imposer avec évidence (soit comme la conclusion d'un raisonnement apparemment juste, plausible, soit comme expérience directement vécue dans la perception), et que cependant un moment de réflexion suffise à montrer qu'elle est manifestement fausse (rotation du carré autour du centre du cercle).

Mais les conflits dans lesquels sont engagés les phénomènes perceptifs sont souvent d'une autre nature que ceux dont nous nous sommes occupés jusqu'ici. Ils peuvent se présenter entre les données perceptives et les renseignements acquis par d'autres voies, observation personnelle, inférences ou communications verbales relativement à la situation en face de laquelle se trouve placé le sujet.

Ainsi, il arrive couramment dans les laboratoires que des étudiants aient examiné des dispositifs expérimentaux, qu'ils aient éventuellement dessiné certaines figures avant de procéder eux-mêmes à des observations dans lesquelles se manifestent des illusions d'optiques ou autres. Et, comme on le sait, la contradiction existant entre la connaissance des



conditions de l'expérience et les données perceptives, n'a guère d'influence sur celles-ci lorsque l'on a affaire, comme il arrive souvent, à des systèmes de stimulations dits coercitifs.

Du reste des cas semblables se rencontrent fréquemment dans la vie courante. Permettez-moi, à ce propos, d'évoquer un souvenir personnel qui m'a vivement frappé. Assistant, il y a nombre d'années à une séance de prestidigitation, je me trouvais à faible distance, à un mètre cinquante au maximum, de l'opérateur, lequel s'était placé au milieu de la salle, et tenait en mains une petite cage de bois contenant un canari vivant. L'illusionniste parlait d'abondance comme cela se fait d'ordinaire en pareilles circonstances, tout en maintenant la cage bien en vue. Puis, tout à coup, il poussa un cri, fit un mouvement rapide des deux bras qu'il tendit brusquement en l'air, et tout avait disparu, cage et oiseau.

J'ai rarement dans ma vie éprouvé une impression aussi vive, aussi évidente d'*annihilation* subite d'un objet fort complexe que je regardais attentivement, bien que depuis lors, nous ayons procédé à des expériences à ce sujet (12). Néanmoins, il va de soi que je n'ai pas cru un instant à pareille annihilation, mais que ma réaction immédiate a été de me demander où le prestidigiteur pouvait bien avoir fait passer cage et oiseau, et comment il avait procédé. Inutile d'insister, car presque tout le monde s'est trouvé un jour ou l'autre dans une situation semblable. Et précisément, parlant récemment de ce cas à un ami, celui-ci m'a raconté qu'il avait assisté avec son fils, âgé d'une dizaine d'années, à une représentation au cours de laquelle un jongleur faisait disparaître successivement une série de balles, puis en faisait réapparaître d'autres, et ainsi de suite. Et la réaction de l'enfant fut caractéristique ; il demanda en effet à son père : « *Mais, le jongleur détruit-il réellement les balles ?* » Il y avait donc déjà un doute, relativement à la valeur de l'évidence perceptive.

Par contre, comme on le sait, et comme les travaux de Piaget, en particulier, l'ont bien montré, les enfants plus jeunes ne voient pas la moindre difficulté à admettre des « créations » ou des « annihilations » de matière, conformément à leurs impressions sensorielles ; et ce n'est que peu à peu, à la suite d'un long processus que se développe et s'affirme la notion de la « conservation de la matière » (13).

Dès que celle-ci est définitivement acquise et que, d'autre part, elle se trouve apparemment contredite par une expérience perceptive, nous sommes convaincus que nous sommes les jouets d'une illusion. Ce sont les connaissances acquises par inférence et par communication verbale qui l'emportent et qui déterminent notre comportement. L'application du concept d'illusion a pour effet de résoudre le conflit, l'opposition, entre

l'évidence immédiate et ce que « l'on sait » par ailleurs. Et ceci est assez curieux car il s'agit en l'espèce d'une notion d'ordre intellectuel et lorsque cette notion est acceptée par l'observateur, cela suffit à rendre inopérante l'impression vivante qui s'impose, malgré tout, à ses sens.

Cependant, il ne faut pas oublier que, comme nous venons de le signaler, ceci est l'aboutissement d'un long apprentissage qui nous amène, progressivement, depuis notre enfance, à *ne plus croire nécessairement* aux données de nos expériences perceptives, ou, plus précisément, à ne pas croire aveuglément à *certaines catégories* d'expériences, ou à celles qui se produisent dans des situations déterminées (au cours de séances de prestidigitation ou de démonstrations relatives à la psychologie de la perception, par exemple).

Seulement, les choses se compliquent de nouveau, du fait que parfois c'est l'évidence perceptive actuelle qui l'emporte, tout de même, et qui détermine la conduite des sujets.

J'ai eu l'occasion d'observer de nombreux cas de cette espèce dans mon laboratoire et ailleurs, et je voudrais en rapporter quelques-uns ici.

Le premier est encore un vieux souvenir qui remonte à l'année 1937, lorsqu'on commença à montrer à Paris dans une salle publique des projections cinématographiques en relief, suivant le procédé Lumière (application des anaglyphes). Au cours de ces représentations qui, pour le reste, se déroulaient sans histoire, on faisait voir un match de foot-ball, et à un moment donné, le ballon semblait sortir de l'écran, et être projeté dans la salle à la hauteur de la tête des spectateurs. Or, à cet instant une quantité de ceux-ci se penchaient brusquement sur le côté, comme pour éviter le projectile. Cette réaction est intéressante, car il est évident que toutes les personnes présentes savaient parfaitement bien qu'il s'agissait d'une illusion, et elles le savaient tellement bien, que c'était justement leur curiosité à ce sujet qui les avaient amenées dans la salle. Et malgré cela, un bon nombre d'entre elles réagissaient comme s'il s'était agi du lancement d'un objet réel. Pourquoi en allait-il ainsi ? Différentes hypothèses sont plausibles. On pourrait supposer notamment que, malgré tout, ces personnes étaient tellement sous l'influence de la réalité apparente du spectacle que le « savoir » qu'il s'agissait de simples images était mis en veilleuse, demeurait latent, et que son efficacité sur le comportement s'en trouvait d'autant réduite. On pourrait insister d'autre part sur le fait qu'il s'agissait en l'espèce d'une réaction « instinctive », non apprise, si fortement liée à l'évidence perceptive que celle-ci devait la déclencher, en dépit de la connaissance de l'illusion. Ceci n'est nullement invraisemblable et il ne serait sans doute pas bien difficile d'étudier la question de façon

systématique, et en particulier de chercher à conditionner une inhibition de réactions de cette nature dans des situations expérimentales, aisées à réaliser.

Mais passons à des observations de laboratoire. J'ai mentionné précédemment, ces objets reproduisant en fils de fer et *en plan* les lignes constitutives d'une représentation perspective d'un parallépipède ; et j'ai signalé que ces objets, regardés en vision monoculaire d'un point de vue déterminé donnaient l'impression d'un volume absolument réel.

Or, j'ai été plus d'une fois témoin d'un singulier comportement de personnes qui faisaient l'expérience, et même d'un homme de science bien connu. Ces sujets connaissaient parfaitement les conditions expérimentales, ils avaient vu le modèle de fil de fer en-dehors de l'appareil, et on leur avait, de plus, donné toutes les explications possibles sur l'illusion. Néanmoins, ils ont voulu répéter l'expérience et contrôler « s'il n'y avait vraiment pas moyen de manœuvrer un objet solide, un crayon par exemple, de telle manière qu'ils auraient l'impression de le faire passer entre les plans limitant *ce volume* qui leur paraissait réel ».

Cette conduite était à tout le moins étrange, car il est clair qu'un simple moment de réflexion aurait suffi à faire comprendre aux observateurs que cette tentative *ne pouvait pas* réussir. Du moment en effet qu'ils savaient (pour l'avoir constaté eux-mêmes) que tous les fils métalliques de la figure se trouvaient dans un même plan, il était évidemment impossible de faire passer, même apparemment, une tige rigide *derrière* deux des fils définissant le plan phénoménal « antérieur » et *devant* les fils définissant le plan « postérieur ». L'essai était donc fatalement voué à l'échec, et l'on se perd en conjectures sur les raisons qui peuvent avoir conduit des personnes de haute valeur intellectuelle, à le tenter. Il s'agit ici sans doute de cas plutôt exceptionnels et l'on peut se demander si parfois l'évidence perceptive n'est pas tellement prenante, qu'elle produit un véritable blocage, une obstruction momentanée, de la pensée. Quoi qu'il en soit, il y a là une énigme qu'il serait intéressant de tâcher de résoudre.

Voici un autre cas, assez différent, mais qui montre lui aussi la puissance éventuelle de l'évidence perceptive. J'avais décrit à mes élèves une expérience relative à la permanence phénoménale, qui se réalise de la manière suivante :

On fait voir aux sujets, derrière une longue fente étroite découpée dans un écran, deux rectangles de couleurs différentes se trouvant à quelques centimètres de distance l'un de l'autre. A un moment donné, le rectangle de gauche se déplace vers celui de droite et, à partir du moment où il est entré en contact avec lui, il commence à diminuer de longueur jusqu'à

disparaître complètement. Quand les conditions expérimentales convenables sont réalisées, les observateurs sont unanimes à affirmer qu'ils voient le rectangle en mouvement glisser derrière l'objet immobile, en maintenant inchangée sa longueur totale (14). C'est « l'effet Ecran » typique.

Mes élèves savaient donc ce qui se passait, la technique suivie, et connaissaient même la théorie du phénomène. Or je constatai que l'un d'eux se rapprochait très près de l'écran, l'examinait avec un soin particulier, et cherchait à contrôler par la vue et par le toucher s'il s'agissait bien en l'occurrence de simples figures colorées peintes sur un disque de papier, et s'il n'y avait nulle part de fente dans ledit papier ! Et quand je lui demandai pourquoi il faisait cela, il me fit une réponse singulière.

S'il m'avait dit simplement que, se trouvant dans un laboratoire de psychologie, on pouvait se demander si tout ce que j'avais expliqué n'était pas une feinte, une tentative de suggestion destinée à faire croire aux sujets qu'un glissement réel n'était qu'une simple apparence, cette réponse aurait été parfaitement sensée, car pareille supposition pouvait être plausible. Mais la réponse donnée fut toute différente ; la voici : « J'ai voulu vérifier ce qui en était parce que ce glissement était si évident, que *je ne pouvais pas croire* que c'était une illusion ».

Inutile de souligner que pareille réponse manquait de pertinence, puisque c'est précisément le fait qu'une impression « évidente » ne correspond pas à la situation physique, qui caractérise l'illusion.

Et pour terminer, je mentionnerai un dernier cas, dans lequel j'ai réagi moi-même d'une façon plus ou moins semblable à celle du sujet précédent. Il s'agissait cette fois des recherches, citées plus haut, que nous avons faites sur la transparence apparente (15). Après avoir acquis déjà une expérience étendue en cette matière, il s'est fait que, procédant un jour à un nouvel essai, j'y réagis en disant qu'il devait y avoir cette fois une erreur expérimentale, *parce que je voyais si clairement le rectangle à travers l'écran, que celui-ci devait certainement être physiquement translucide*. Je savais cependant que le Dr. Glynn était un expérimentateur consciencieux et soigneux ; mais, évidemment, il peut arriver à n'importe qui de se tromper parfois ; aussi procédâmes-nous immédiatement à l'examen de l'écran, qui était de fait absolument opaque.

Dans les trois exemples décrits, c'était chaque fois l'évidence perceptive qui s'est montrée dominante et déterminante de la conduite des sujets. Les deux derniers cependant me paraissent particulièrement instructifs à différents points de vue.

D'abord, ils font ressortir le fait que si nous apprenons progressivement

à nous méfier de nos impressions sensorielles et à acquérir le concept d'illusion, ceci n'aboutit pas à un scepticisme universel parce que cet apprentissage est réellement *sélectif*. Le concept d'illusion n'est appliqué en général qu'à des cas ou à des domaines où nous avons appris par expérience, qu'il y avait souvent une opposition entre les impressions et les situations physiques correspondantes. Ainsi, la vie courante nous a enseigné que nos impressions de distance, de grandeur, même de forme, nos impressions de couleurs, sont souvent trompeuses, et nous sommes tout prêts à admettre qu'il s'agit d'illusion dans ce domaine, dès qu'on nous le dit. Il en va tout autrement quand on a affaire à des cas nouveaux dont on n'a pas eu d'expérience préalable. Alors les sujets se comportent comme des enfants, ils « ne peuvent pas croire que c'est une illusion » ainsi que nous venons de le constater.

Personnellement j'étais *pratiquement certain* qu'il y avait erreur dans l'agencement de l'expérience. Et cependant, le scepticisme dont je viens de parler est tout de même plus général qu'on ne pourrait le croire, car, malgré l'évidence perceptive, il devait y avoir une trace de doute chez moi, puisque j'ai tenu à vérifier l'appareil utilisé. Il est vraisemblable que chez l'adulte cultivé du moins, la foi dans les données perceptives seules n'est jamais absolue, tout au plus peut-elle créer une présomption d'erreur dans les connaissances contradictoires acquises par d'autres voies.

En tout état de cause, dans tous ces cas de discordance, on se trouve en présence du dilemme : *illusion* des sens, ou *erreur* dans nos raisonnements, ou dans nos mesures, ou dans ce que nous disent d'autres personnes relativement à la situation physique. Et la question est de savoir de quel côté penchera la balance et pourquoi.

Aussi, si l'on me demandait maintenant quelle est mon opinion au sujet de la question de l'importance relative de l'évidence perceptive et des connaissances acquises dans la détermination de notre conduite, en fonction du monde physique, ma réponse devrait-elle être que la question est mal posée en ces termes, parce que cela dépend d'autres conditions.

Il n'est pas absurde, nous l'avons vu, de supposer qu'un comportement instinctif, non appris, puisse être déclenché directement par la perception, bien que l'on « sache » que celle-ci soit illusoire. Mais quand il s'agit d'autres comportements, appris au cours de la vie, c'est, semble-t-il, dans la mesure où l'on croit que l'une des sources d'informations est plus que l'autre conforme à la situation par rapport à laquelle on doit agir, que celle-là est déterminante. Ce sont donc *les degrés de croyance* ou les degrés de certitude relatifs qui sont l'essentiel, et par conséquent, en dernière analyse, *les motifs* qui les justifient. Il faut noter toutefois que

l'évidence perceptive, lorsqu'elle est confrontée avec une évidence logique discordante fournie par un contrôle adéquat, cède toujours le pas à cette dernière, ce qui fait ressortir en l'espèce la prééminence de la pensée sur les données perceptives comme telles.

Quoi qu'il en soit de ce dernier point, tout ceci montre que *l'événement perceptif* envisagé dans toute sa complexité comporte toujours, chez l'individu qui a atteint un certain niveau de développement, un côté personnel d'acceptation, de refus, de foi ou de doute, qu'il est indispensable de prendre en considération dans l'étude du comportement.

Et ceci me fait songer à un article publié il y a quelques années par Klein et Schlesinger, article dont le titre avait un peu la résonnance d'un slogan : « Where is the Perceiver in Perceptual Theory ? » (16).

Je pense que ce qui a été discuté dans ces pages met en relief au moins l'un des aspects de la présence de celui qui perçoit, dans la « perception ».

#### LITTÉRATURE

- (1) Voici quelques indications relatives à un certain nombre de ces travaux.
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  - E. L. KNOPS, Contribution à l'étude de la « naissance » et de la « permanence » phénoménale dans le champ visuel. Miscellanea Psychologica Albert Michotte. Louvain, Publications Universitaires de Louvain, 1947, pp. 562—610.
  - F. A. MICHOTTE, A propos de la permanence phénoménale. Faits et Théories. Acta Psychologica Vol. VII, 1950, pp. 298—322.
  - G. A. MICHOTTE, L'énigme psychologique de la perspective dans le dessin linéaire. Bulletin de la Classe des Lettres de l'Académie Royale de Belgique, 5ème série, Vol. XXXIV, 1948, pp. 268—288.
  - H. M. R. PREMISTER, An experimental Contribution to the Problem of apparent Reality. Quarterly Journal of experimental Psychology. Vol. III, 1951, pp. 1—18.
- (2) Des cas de cette dernière espèce ont donné lieu à toute une série de travaux, parmi lesquels, notamment, ceux, bien connus, de Bruner et Postman. On trouvera des indications générales à ce point de vue dans : M. D. VERNON, A further Study of Visual Perception. Cambridge University Press 1952, Chapter X (4).
- (3) Comme on le voit, le terme « processus perceptif » est pris ici dans un sens très large qui recouvre aussi le système de stimulations. Ceci nous paraît

indispensable car il semble que ce soit le seul moyen de donner une définition susceptible de s'appliquer à tous les cas que nous avons en vue. Bien souvent en effet, l'opposition, la contradiction, ne se manifestent nullement au sujet comme fait observable, et ce n'est qu'une étude comparative des effets de différents systèmes de stimulations qui les révèle à l'expérimentateur.

- (4) Voir à ce propos : L. BURKE, The Tunnel Effect. *Quarterly Journal of experimental Psychology*, Vol. IV, 1952, pp. 121—138.
- (5) Consulter l'article indiqué au G de la note (1) et aussi A. MICHOTTE, Le caractère de « réalité » des projections cinématographiques. *Revue Internationale de Filmologie*. Vol. I, 1948, pp. 249—261.
- (6) Consulter l'article indiqué au H de la note (1). Les figures dont il s'agit ici étaient les mêmes que celles des photographies utilisées par Phemister, et elles étaient présentées sous l'angle qui donnait, d'après les résultats de cet auteur, l'impression la plus forte de réalité.
- (7) C'est avec raison, croyons-nous, que Rubin signale une *perte de réalité apparente* dans ses expériences (E. RUBIN, *Visual Figures Apparently Incompatible with Geometry*. *Acta Psychologica*, Vol. VII, 1950, p. 269). Cette *perte* du caractère phénoménal de réalité nous semble se présenter très souvent dans les cas d'incompatibilité et singulièrement dans le cas précité de l'hémisphère.
- (8) Cité par W. METZGER, *Gesetze des Sehens*. Kramer, Francfort, 1953, p. 130. On trouvera dans le même ouvrage (pp. 334 et 323) des renseignements sur des expériences faites par d'autres auteurs, mais prochainement apparentées à celles qui sont décrites dans les pages suivantes.
- (9) A. GLYNN, Apparent Transparency and the Tunnel Effect. *The Quarterly Journal of Experimental Psychology*, Vol. VI, 1954, pp. 125—139.
- (10) F. P. KILPATRICK, Two Processes in Perceptual Learning. *Journal of Experimental Psychology*, Vol. 47, 1954, pp. 362—370.
- (11) Cependant des expériences de contrôle ont montré que la même déformation visuelle se manifestait lorsqu'on mettait, après l'apprentissage, les sujets en présence d'une chambre réellement orthogonale ou d'une autre chambre déformée produisant une image rétinienne semblable. Ce fait démontre qu'il s'agit bien, fondamentalement, des effets de l'apprentissage sur la structure perceptive correspondant à un même système global de stimulations.

Je profite de cette occasion pour adresser mes vifs remerciements au Professeur Kilpatrick pour la courtoisie qu'il a eue de me prêter les épreuves d'imprimerie de son travail, qui n'avait pas encore paru à l'époque, et de m'autoriser à en faire usage pour ma conférence.

- (12) L. KNOPS, Contribution à l'étude de la « naissance » et de la « permanence » phénoménales dans le champ visuel. *Miscellanea Psychologica Albert Michotte*. Editions de l'Institut Supérieur de Philosophie de Louvain, 1947, pp. 562—610.
- (13) J. PIAGET et B. INHELDER, Le développement des quantités chez l'enfant. Delachaux Neuchâtel, 1941, pp. 81 seq.

Voir aussi J. PIAGET, La soustraction des surfaces partielles congruentes à deux surfaces totales égales. *Miscellanea Psychologica Albert Michotte*. Editions de l'Institut Supérieur de Philosophie de Louvain, 1947, pp. 167—180.

- (14) Consulter les articles indiqués au D et au F de la note (1).
- (15) A. GLYNN, loc. cit.
- (16) G. S. KLEIN et H. SCHLESINGER, Where is the Perceiver in Perceptual Theory? *Journal of Personality*, XVIII, 32, 1954.



## ABSTRACTS OF PAPERS / RÉSUMÉS DES COMMUNICATIONS

(*Editor's note.*—In addition to formal addresses, the programme consisted of symposia and papers organized about a variety of topics of current interest. The abstracts of these papers are presented here under the heading of each symposium in the order in which they were given.)

(*Note de la rédaction.* — En plus des conférences du soir, le programme comprenait des symposiums et des communications portant sur une variété de sujets d'intérêt actuel. Les résumés de ces communications sont publiés ici sous le titre de chaque symposium d'après leur ordre de présentation.)



TUESDAY, JUNE 8 — MARDI LE 8 JUIN

## SYMPOSIUM

### EXPERIMENTAL AND THEORETICAL ANALYSIS OF INSTINCTIVE BEHAVIOR

### ANALYSE EXPÉRIMENTALE ET THÉORIQUE DU COMPORTEMENT INSTINCTIF

*Chairman — Président*

FRANK A. BEACH (Yale)

*Discussion*

FRANK A. BEACH (Yale) — E. H. HESS (Chicago)

*Papers — Communications*

### EGG RECOGNITION IN THE HERRING GULL

BY

G. P. BAERENDS (Groningen)

Research on birds, fishes and insects has revealed interesting characteristics of the external stimulus situation necessary to evoke a motor response. It has been shown in a number of cases that only a few of the physical stimuli presented by the situation and received by the reactor are necessary to release an activity. The effective stimuli differ in their releasing values. The total releasing value of the situation is determined by combining the values of the different components. This so-called "law of heterogeneous summation", formulated first as a tentative hypothesis by Seitz, should be checked and eventually amended through careful quantitative studies of many different cases.

The recognition of the egg by the herring gull presents a suitable case. Actually the gull has two completely different ways of reacting to an egg;

it may eat it or it may treat it as an object for incubation. We will deal here with brooding activities only and will restrict ourselves to our study of the values of different characters of an egg that has rolled out of the nest.

All experiments have been done in the field in a colony of wild birds. Wooden models were placed on the nest rim and the reaction of the bird after it had come back to the nest was observed from a hide at a short distance from the nest. After having compared in preliminary experiments the advantages and disadvantages of offering different models simultaneously or successively we decided in favour of choice experiments in which the bird had to choose between two different egg models every time. Till now only the characters "spottedness" (in collaboration with J. P. Kruyt) and "size" (together with G. J. Blokzijl) have been tested in large series of experiments.

It could be proved that spotted eggs have a much higher releasing value than evenly coloured eggs. The releasing value could be increased by painting more spots on the models, by making the spots smaller and by increasing the contrast between the spots and the ground colour. Size was of great importance; in general, the greater the size of the model the greater its releasing value.

With regard to spottedness as well as to shape it was possible to make models very different from normal eggs but with a much higher (supra-normal) releasing value.

To obtain these results with satisfactory significance in quantitative series we had to carry out a great number of experiments because of a considerable number of choices for suboptimal models in both series. We have tried to analyze the causes of these minority choices. Two major groups can be distinguished. First, in a number of cases the gulls do not show any preference for one of the models. Then they may either choose just at random (such birds are not at all afraid of the experimental setup and show signs of high brooding drive) or they may have a position preference. The latter seems to increase in the course of a longer series; it can be broken by increasing the difference between the models. Secondly, there are cases in which the gulls definitely prefer suboptimal models. Those birds show signs of fear and of a low brooding drive.

AN ATTEMPT AT AN ANALYSIS OF THE PARENTAL  
BEHAVIOR OF THE MALE THREE-SPINED STICKLEBACK

BY

J. J. A. VAN IERSEL (*Leyden*)

The reproductive cycle of the male three-spined stickleback (*Gasterosteus aculeatus* L.) can be divided into three phases, each of which is characterized by the appearance, at a high frequency, of various activities forming a functional unit. We have (1) the nest building, (2) the sexual, and (3) the parental phase.

Factors which affect the transition of the sexual phase into the parental phase were studied experimentally. Therefore, measurements were taken determining the height of the sex and the parental drive on each day of the parental cycle. The height of the drives (the groups of specific internal factors responsible for the performance of an activity) was measured with the aid of an external stimulus of constant strength. The development of the drives was measured under normal conditions and under experimental change of number of fertilizations and of number of clutches.

It was found that the eggs release and prime the parental drive and thus start the parental cycle, which is characterized by an ever increasing performance of the so-called fanning activity: a ventilation movement. The effective stimulus from the eggs is produced by their breathing activity. Both a surplus of  $\text{CO}_2$  and a lack of  $\text{O}_2$  in the nest release the fanning activity.

In the course of the cycle the development of the drive becomes more and more autonomous and less responsive to the increasing stimulation of the growing eggs. This autonomous rhythm, expressing itself in a regular increase of fanning before and a decrease after the hatching of the eggs, can only be built up when the drive grows fast enough. Even when no hatching occurs, after exchanging old eggs for fresh clutches, the autonomous rhythm may be apparent. However, the priming influence of the eggs cannot be dispensed with entirely. If the eggs are taken away the cycle is usually broken off after some days.

Fertilization facilitates the build-up of the fanning drive and temporarily depresses the sex drive. Both effects increase with the increasing number of fertilizations. The influence of fertilization on the sex drive is a direct one. It causes a considerable immediate drop in sex drive, which recovers after some days. Whether fertilization in addition stimulates the fanning drive directly could not be decided.

The priming influence of the eggs on the fanning drive is a direct one. It is improbable that they do influence the sex drive directly.

The sex drive exerts an inhibitory effect on the fanning drive, at least as long as it is of normal strength. The fanning drive exerts an inhibitory effect on the sex drive at least when it has reached a certain level. The development of the fanning drive is favored during the first days of a cycle, because of the temporary depression of the sex drive by the fertilizations. It then reaches a level which inhibits the sex drive gradually, in this way preventing its recovery. The effect of this mutual inhibition is that in normal cycles the sex drive shows a sharp drop at the time when the parental drive ascends steeply; this turning point can present itself earlier depending upon the number of clutches and of fertilizations.

## PARENTAL BEHAVIOR IN BIRDS AND THE PROBLEM OF "INSTINCT"

BY

DANIEL S. LEHRMAN (*Rutgers*)

The parental feeding behavior of ring doves consists primarily of the regurgitation of "crop milk" by the parent to the squab. The crop milk is produced by the crop wall under the influence of prolactin secreted by the bird's pituitary gland. Prolactin injected into birds that have had breeding experience causes them to feed squabs placed in their cages by the experimenter. If the birds have not had breeding experience they will not feed such squabs, even though injected with prolactin. Doves with breeding experience, and injected with prolactin, may be prevented from feeding or approaching squabs by local anesthetization of the crop. Comparison of the normal regurgitation-feeding behavior of doves breeding for the first and for the second time indicates that feeding is elicited at first only by tactual stimulation of the breast (over the crop) of the sitting bird by the head of the squab, and only later by visual and/or auditory stimuli coming from the squab.

These data suggest that the hormone acts primarily by activating the crop, rather than on a central nervous mechanism specific for the feeding act. The activated crop appears to act as a drive state, causing appetitive restlessness which is reduced when the animal performs an appropriate act, which may be learned.

Data from other birds suggest that the establishment of broody (in-

cubating) behavior by prolactin injection may depend upon the hormone's ability to cause development of the brood patches. Again, this is by contrast with an explanation in terms of the hormone's effect on a "center" specific for the behavior.

These data imply certain questions about the validity of attempts to explain such behavior patterns on the basis of centrally-performed "instincts" which organize the act and determine, in advance, its relationship to relevant stimulus situations.

The concept of "drive state" allows a much broader and more subtle consideration of the behavioral effects of hormones than does the dichotomy between "central nervous effects" and "peripheral effects initiating sensory impulses." This type of dichotomy is usually implied by instinct theories which make rigid distinctions between centrally-preformed "elements" of behavior and peripherally-influenced "components", but it is not strongly supported by physiological evidence.

The source of motivation for this behavior, on the basis of these and similar studies, is seen to be not primarily accumulation of central neural excitation, but peripheral physiological changes. It is believed that the concentration of instinct theories upon central sources of motivation often leads to the neglect of other aspects of the organism which, in many cases, may be more relevant.

## SYMPOSIUM

### PROJECTIVE TECHNIQUES AND PSYCHOLOGICAL THEORY TECHNIQUES PROJECTIVES ET THÉORIE PSYCHOLOGIQUE

*(Jointly sponsored by the International Union of Scientific Psychology and  
the Society for Projective Techniques)*

*Chairman — Président*

JOHN BELL (*Clark*)

*Discussion*

JOSE LASAGA (*Habana*), JOHN BELL (*Clark*), C. ROGER MYERS (*Toronto*)

*Papers — Communications*

### PROJECTIVE TECHNIQUES IN THEIR RELATION TO PSYCHO- ANALYTIC AND PSYCHOLOGICAL THEORY

BY

ELSE FRENKEL-BRUNSWIK (*California*)

Unmasking the face-value of behavioral and verbal manifestations, early psychoanalysis has utilized phenotypical characteristics merely as cues for inferences concerning latent (genotypical) forces of motivation. Indirect techniques involving "projection" were invoked as an access to the hidden tendencies.

It will be shown that inferences concerning central and unconscious processes, be they theoretical constructs introduced by postulates or "dispositional predicates" defined by reduction chains, are scientifically legitimate provided that they are based on a wide variety of circumstantial evidence. Dreams, free associations and projective materials provide their empirical interpretations and the links to be inserted in the gap between the conscious and unconscious systems.

Some time ago this writer found that projective material could be predicted or postdicted more effectively from synoptic motivational interpretations of overt behavior than from overt behavior segments taken at face value. The motivational ratings showed "either-or" type of statistical relationship to the manifest overt behavior cues—be these gross or minimal—one manifestation substituting for another in spite of phenotypical dissimilarities or even seeming contradictions. This rational reconstruction of the intuitive inferences by the specification of their relation to the behavioral cues suggested that the organism functions in the manner of a multiple correlation instrument. In the more recent literature we find claims that prediction from projective material to overt behavior often fails. The blame must be laid on an underrating of the complexity of the relationship between underlying tendencies and overt behavior by the investigator rather than on an inherent misleadingness of projective materials or on an inability of the organism to synthesize data of great variety. As in the early stages of psychoanalysis, the regrouping of the manifest data is often done exclusively in terms of "causes" or "needs", ignoring the intricacy of their ramifications in the behavioral effects.

The recent rediscovery of consciousness in psychoanalysis, together with the revival of the traditional emphasis on consciousness and on manifest data in psychology proper, imply increasing emphasis on the diagnosis of the ways of the cognitive mastery of reality. Projective materials are thus viewed not only as revelations of defense mechanisms but as expressions of personal styles of cognitive and perceptual adaptation. For example, this writer has given increased attention to such "formal" characteristics of projective material as intolerance of ambiguity, exaggeration, overconcreteness and overgenerality, small range of variability, repetition, lack of realism and of logical consistency, fluidity and rigidity, overcertainty and compulsive doubt, along with originality vs. stereotypy. Formal indices of the adjustment structure, being probably less subject to censorship and disguise than is the dynamic need content, promise to provide added diagnostic value relative to motivation and its "genuineness". There is evidence that specific contents tend to be associated with specific formal characteristics. Such depth-surface relations are revealed, among others, in the differential formal organization of Thematic Apperception Test stories as it relates to the different stages of psycho-sexual development assumed by psychoanalysis. To assure an adequate basis for long-range prediction of the most relevant—though perhaps not always the most conspicuous—aspects of behavior, the inferences as to the underlying motivation must be recombined with the phenotypical characteristics of

the story told. Synoptic analysis of the composite clinician's patterns of observation and extrapolation will also help to make explicit interpreter bias.

## PSYCHOLOGY OF MENTAL CONTENT RECONSIDERED

BY

DAVID C. MCCLELLAND (*Wesleyan University*)

The psychology of mental content almost disappeared under the impact of behaviorism, the apparent fruitlessness of Titchener's introspectionism, and the American emphasis on action rather than thought or feeling. Even in personality and social psychology there was little interest in content variables. Instead, psychologists became almost wholly concerned with processes, with thinking, learning, perceiving, etc. There was little concern with *what* was learned, *what* was perceived, or *what* was being thought.

During this time clinical psychologists had to remain interested in what their patients were thinking, and they developed as tools projective tests which enabled them to study thought content more systematically. Thus work on mental content continued almost entirely outside the academic tradition until the development of the Thematic Apperception Test by Murray which then combined with the most successful psychology of the clinic, namely psychoanalysis, to give a new impetus to the study of mental content.

By concentrating on one type of imagery centering around success or achievement in a modified form of the Thematic Apperception Test, we have managed to open up for scientific study a whole new set of relationship between religious values, independence training, achievement motivation and economic development. In this way, psychology may once again be able to make direct contributions to such content-oriented social sciences as History, Political Science and Economics.

The new approach differs from introspection in that it samples the thoughts of individuals without asking them to judge or categorize them. Thus the data are public and can be classified by any number of individuals according to any system of categories of theoretical relevance. The method involves many problems, such as when to sample thoughts, how to collect them, and what categories to use, but these are solvable difficulties and need not interfere with its wide application to studying mental content scientifically and objectively. For the most part, we have



to thank the projective testing movement for keeping alive this interest in content when academic psychology was otherwise engaged, and for providing us with a way out of the blind alley of self-description which seriously limited the effectiveness of introspection as a method.

## PROJECTIVE TESTS AS RESEARCH TOOLS

BY

DANIEL R. MILLER (*Michigan*)

The contributions of projective tests to psychological theory are highlighted when they are used to verify experimental hypotheses. This paper reports the experience of members of a research project who have used projective instruments as one of their major sources of information concerning defense mechanisms.

It has been established that certain mental disorders occur disproportionately in the middle and lower socioeconomic levels. We assumed that each disorder is a result of a specific pattern of defence against contradictory impulses. We deduced that members of these two classes must originally have had different learning experiences which predisposed them to handle similar conflicts in terms of different defenses. In our experiments, we typically measured relationships between background variables such as parental discipline or social class and the current reactions of adolescent boys to conflict.

We soon found it more practical to reject current tests in favor of techniques that could be tailored to our needs. Initially we selected a story completion test. Because of our many control variables we had a fairly small sample. It was necessary to have relatively structured items so that we could obtain information relevant to our theory from most subjects. Other criteria not met by traditional projective methods are also mentioned. It also became important to spend some time investigating scoring for its theoretical meaning, sufficient spread, and cutting points for groups. Finally we had to match our story beginnings for equivalence of stimulus value. None of the traditional techniques for eliciting defense met all our criteria.

In the first study we obtained projective endings to story beginnings in which heroes were angry at authority figures. But the responses contained little defensive distortion and they agreed closely with peers' ratings of actual aggressive behavior in everyday social situations.

This challenged us to consider the conditions under which we might

obtain maximal defensive distortion. We decided that the identification of a defense requires, at a minimum, evidence of the forbidden impulse in a form not recognized by the individual, and independent conscious behavior that belies the impulse. The initial study also emphasized that the forbidden need must reach a certain strength before the person has to defend against it.

With the aid of this theory, we designed a new experiment. A projective test was first administered under a neutral condition in which the need would presumably be weak. Then we provoked aggression and gave a matched projective test when the need presumably was strong. The discrepancy between the initial and final testing yielded defensive distortions which relate significantly to background criteria in the manner predicted. We obtained similar success with defenses against failure as measured by a test of the achievement motive.

Some of the implications of our experiences with the projective tests in our studies are discussed in terms of: (1) the need to design special projective instruments for different theoretical problems; (2) the fallibilities of some current assumptions about projective tests and techniques for identifying defenses; and (3) some of the theory that is implicit in the structure and interpretation of any projective test.

The Editors regret that Dr. D. J. VAN LENNEP's summary is not included in these Proceedings.

This paper will be published *in extenso* in DAVID, Current Perspectives in Personal Theory (in preparation).

## SYMPOSIUM

### MULTIVARIATE METHODS FOR ELECTRONIC COMPUTERS MÉTHODES DE CALCUL COMPLEXES AU COMPUTATEUR ÉLECTRONIQUE

*Chairman — Président*

LEE J. CRONBACH (Illinois)

*Papers — Communications*

#### SOME PATTERN ANALYTIC METHODS

BY

LOUIS L. MCQUITTY (Illinois)

*Purpose:* These studies develop and illustrate methods of analyzing responses to psychological tests. The methods have the following characteristics: (a) applicable to categorized data, (b) admit all kinds of interrelationships to the analysis, (c) indicate whether the data can be explained best in terms of dimensional, typological, or typo-dimensional constructs, (d) classify patterns of responses into categories so that each pattern resembles its category more than it does any other pattern, or any group of patterns, not in its category, (e) can assess patterns of responses in relation to a complex criterion.

*Method:* A pattern of responses is the answer which one or more individuals gave to specified items of a test. An individual pattern of responses is the answer given by a particular individual to specified items.

The methods classify each individual pattern into a species so that its pattern is more like the pattern of its species than like that of any other individual, or group of individual patterns not in its species. Each species defines a species pattern. Species patterns are then classified into genera, and genus patterns are realized. Higher order classifications are obtained similarly.

A psychological construct is inferred to explain each pattern of responses. An analysis indicates whether these constructs can be explained

best as dimensional, typological or typo-dimensional. At the genus level, for example, an index of correlation is taken between each genus pattern with every other genus pattern. This index is analogous to the one between persons over items used in the Q-technique of factor analysis.

If the genus patterns are all relatively independent of each other, the inferred constructs are then best interpreted as typological differentiae. If, however, the correlations tend to be one, either plus or minus, except for differences in popularity of some of the responses, then the inferred constructs are best interpreted as dimensional.

If any two genus patterns can be found which have a high negative correlation, they tend to define a bipolar dimension; their inferred constructs can best be interpreted as typo-dimensional.

In the course of the analysis, a search can be made for (a) psychological differentiae peculiar to levels of standing on a complex criterion, (b) dimensions significantly related to the criterion and (c) typo-dimensions related to the criterion. These latter dimensions provide that each subject can be evaluated in terms of the particular typo-dimension most appropriate to him. Each subject is first classified with the category of subjects whose psychological differentiae he most resembles. He is then scored on that typo-dimension which is formed by joining this psychological differentiae with the one most opposite it. This approach recognizes various ways of succeeding and failing in reference to a complex criterion and attempts to evaluate each subject in terms of the way most appropriate to him.

*Results:* Equations were developed to analyze patterns and were applied to data which yielded psychological differentia and typo-dimensions related to criteria of psychological well-being and mechanical success. They have been adapted to an electronic computer.

## TESTS OF SIGNIFICANCE IN FACTOR ANALYSIS

BY

C. RADHAKRISHNA RAO (*India*)

It is shown that factor analysis is an investigation into the relationship between two sets of variables, one of which is observable (scores secured by an individual in a battery of tests) and the other hypothetical (the set of factors causing the correlations between test scores). The number of independent hypothetical factors is inferred to be the number of significant canonical correlations between the two sets of variables.

The test criteria for determining the significant correlations are derived

by the method of maximum likelihood. They depend on the roots of a determinantal equation of the form

$$(GRG - \lambda I) = 0$$

where  $R$  is the observed correlation matrix and  $G$  is a diagonal matrix depending on the 'communalities'. Since  $G$  is unknown, additional equations are provided for their estimation.

The computations are not, however, simple. It is hoped that high speed computers will come to our rescue.

## POSSIBLE USE OF ELECTRONIC COMPUTERS IN MENTAL TESTING \*

BY

LEDYARD R. TUCKER (*Princeton*)

While only limited use has been made of electronic computers in mental testing operations to date, an expansion in application of these machines is indicated for the near future by several developments now under way. A projection into the more distant future points to an increasing dependency on electronic automata in performing not only the more routine data processing activities but also computations associated with more intricate analysis methods applied to mental testing results.

Present use of electronic machines for mental testing has been limited to experimental analyses. An early development was the construction of a special device, the Matrix Rotator, for the Personnel Research Branch of the Adjutant General's Office, United States Army, to assist in graphical rotation of axes in factor analysis. General purpose electronic computers have been used to determine the characteristic vectors and roots of several symmetric matrices as portions of factor analysis studies. The Whirlwind computer was used by Frederic Lord to compute a factor analysis of 33 tests to ten factors by D. N. Lawley's original maximum likelihood method for factor analysis. David Saunders and Charles Wrigley have used the Illiac computer for an objective method of rotation of axes in factor analysis. In a study conducted by the author on the establishment of a scale of level of proficiency in vocabulary, a card programmed calculator was used for many of the computations.

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An exciting development for the operation of mental testing activities is the construction of new scoring machines. Two large scale scoring machines have been announced. Elmer J. Hanks has constructed a machine involving photoelectric reading of answer sheets and electronic processing of this information to printed scores. A second large machine is under construction for E. F. Lindquist. This machine will also use photoelectric reading of answer sheets and electronic processing of the data and printing of scores. In addition, this machine may be used to cumulate sums of scores and sums of products of scores for two tests over a sample of examinees. It is anticipated that such machines will materially alter the testing operations for large scale examining programs.

A contrast might be drawn between present methods of analysis used in mental testing and those that might be developed for use with electronic computers. With these machines we may free ourselves from some of the present restrictions on analysis methods and make use of more intricate procedures which may reveal more clearly some subtle effects in mental testing.

## IMPACTS OF HIGH-SPEED ELECTRONIC COMPUTERS ON PSYCHOLOGICAL RESEARCH

BY

CHARLES WRIGLEY (*Illinois*)

This paper will consider the extent to which electronic computers are likely to be useful to psychologists, in the light of experience gained in the use of Illiac (the University of Illinois electronic computer) with psychological problems. The main point will be that an electronic computer makes multivariate calculations no more time-consuming than traditional univariate calculations have been with more usual computational aids.

Electronic computers have already revolutionized numerical work in the physical sciences. Although hourly costs are high, these new machines are of such high speed and great accuracy that lengthy calculations can be made more cheaply on electronic computers than in any other way, so that the number of installations has recently increased rather rapidly.

Their high speed is as useful to the statistician as to the physicist, bringing much which has been regarded as purely theoretical within reach of application. There is currently a highly developed body of knowledge in multivariate analysis, applicable for the most part to psychological problems. These multivariate methods generally require the basic algebraic

operations of matrix multiplication, matrix inversion, and the calculation of latent roots and vectors. Using conventional computational methods, these matrix operations are dishearteningly laborious, so that procedures such as canonical analysis and the multiple discriminant function have hardly been used by psychologists, although apparently well-suited to the study of predictive problems. Now that sets of machine orders for the basic matrix algebra operations have been prepared for the Illiac, most multivariate procedures no longer present any great computational problem. Once experience is gained in the use of these methods, we may expect that the psychologist will try to develop more realistic and more complex mathematical models, e.g., non-linear relationships, multivariate patterns. This implies that he will have to cooperate more closely with mathematical statisticians than hitherto, so that there is increased need for improved mathematical training for psychologists.

Factor analysis is the branch of multivariate analysis to which psychologists have mostly restricted themselves. They have displayed great ingenuity in developing serviceable methods for desk calculators, but these are generally so dependent upon the exercise of human judgment (e.g., the conventional methods of rotation) as to be ill-suited for an electronic computer. There will be a description of an analytic procedure for rotation and new procedures for estimating communalities and matching factors which have been devised to supply completely objective methods for the Illiac.

Reference will then be made to our Illiac experience, and particularly to the problems of the transitional period such as getting a trained staff and writing the sets of machine orders.

The long-term prospect is for a drastic reduction in arithmetical drudgery, leaving more time to the psychologist for his main function of formulating and testing hypotheses. The principal problem at the present time is for the psychologist to get access to these new machines. It would be of inestimable value to have an electronic computer installation expressly designed for psychologists, and available on a national or possibly even an international basis.

## SYMPOSIUM

### THE RELATION OF THE PERSON TO HIS ENVIRONMENT RAPPORTS ENTRE L'INDIVIDU ET SON ENTOURAGE

*Chairman — Président*

GEORGE HUMPHREY (*Oxford*)

*Discussion*

WOLFGANG METZGER (*Münster*), S. K. MITRA (*India*)

*Papers — Communications*

#### "RATIOMORPHIC" MODELS OF PERCEPTION AND THINKING

BY

EGON BRUNSWIK (*California*)

Bodies of information may be treated enumeratively, as in geography, or nomothetically, as in physics. Pending added geographic "control" of relevant environmental conditions, physical laws allow dependable prediction from a single cue for each constituent variable ("certainty-geared interaction").

Sampling of geographies from a circumscribed but otherwise uncontrolled natural-cultural habitat or "ecology" as the reference universe, acts as a vague substitute for direct control of the medium allowing "blind" probability learning of stereotyped regularities of limited statistical validity. Predictability in ecological textures may be improved by multiple systems of mutually substitutable, or "vicarious", cues, with resultant cue-rivalries and compromises ("probability-geared interaction").

The varieties of the utilization and/or distortion of these information potentials by a responding organism are best approached objectively by means of functional behaviorism rather than introspectively. By recognizing "perception" as predominantly probability-geared and "thinking" as predominantly certainty-geared, these presumed major sub-systems of the cognitive apparatus may be operationally redefined. A cognitive task



approached once by typical intuitive perception and once by typical explicit ratiocination is used as a concrete illustration, and the persons' strategy is projected upon their achievement in establishing correct environmental relationships ("psychology in terms of objects"). For perception, multiple-track check-and-balance mediation is revealed by a relative paucity of on-the-dot precise responses counterbalanced by a relatively organic and compact distribution of errors free of gross absurdity. For ratiocination, single-track strategy proves highly vulnerable in practice; inadvertent task-substitutions and other derailment-type errors often reaching bizarre proportions belie the relatively large number of absolutely precise responses.

Perception and thinking thus emerge as different forms of imperfect inferences regarding the environment, subsumable to a common behavior model patterned upon reasoning ("ratiomorphic" reduction, if this Latin-Greek hybrid be permitted). Turning to cybernetics for further explanation, "thinking" resembles the standard use of calculating machines whereby the representational control of the input is tacitly entrusted to the extraneous human operator and single-track nets appear sufficient for the remaining, chiefly deductive leg of the construction process, with switching errors inside the machine as the major source of failure. Intuitive perception resembles inductive cybernetic "predictors", or, still better, telecommunication under partial ignorance of the extrasystemic medium for which "redundancy"—which is essentially vicarious mediation—is the only countermeasure.

The rising importance of ratiomorphic models in the literature on mathematical biophysics, on the theory of emotion and motivation, and on clinical intuition is pointed out. At least for perception, gestalt psychology is interpreted as concentrating on achievementally relatively neutral or even negative dynamisms ("pregnance"; illusions) which in spite of their potency within the intrasystemic field are but side-aspects of uncertainty-gear interaction, and as oblivious of the "associationistic" requirements imposed upon perception by the probabilistic character of its environment—extrapolative functions; furthermore, it may well be ratiocination rather than perception that produces the most erratic forms of sudden change or reversal. A somewhat indirect point of agreement with gestalt psychology is the vindication of the more primordial cognitive functions against the rationalist's overvaluation of the intellect.

COMPLEX INTERMEDIATE PROCESSES BETWEEN SITUATION  
AND RESPONSE: THEIR METHODOLOGICAL IMPLICATIONS

BY

ROBERT LEEPER (*Oregon*)

In the same way that psychology has profited by developing technical concepts about experimental design, perhaps psychology likewise may profit from a more careful development of its "methodology of conceptualizing or explaining".

One means of understanding more clearly how this problem has been approached by different psychologists is to relate our discussion to the experiment which Brunswik's paper describes on perception vs. thinking.

In his own earlier papers, Brunswik has urged a methodology analogous to the perceptual approach in his experiment. He urged that we develop psychological laws in terms of correlations between representative sets of stimulus-situations and related behavior-products. He urged that we avoid studies trying to discover how situations produced results.

Except that the matter of representativeness often has been neglected, this approach actually has been used much more widely than Brunswik seemingly has realized. Innumerable studies of individual differences, industrial psychology, education, and sociology have been of this sort.

Such studies usually have revealed low correlations between such distal factors, but possess some value anyway. For example, it is worthwhile to an insurance company to know that college professors have fewer automobile accidents than lawyers or ministers or vaudeville performers. However, the important task for psychology is not just the discovery of such rough means of prediction. More truly it is the development of those principles and means of measurement which will permit maximally precise predictions of the behavior and experience (a) of one individual as contrasted with another, and (b) of the same individual under different circumstances.

To achieve such precision, we must follow a method analogous to the "thinking" approach in Brunswik's experiment. To do this, if we are to use a broad range of experimental data, we must state relationships via factors and processes within the organism. As Brunswik's own experiment indicates, low distal-distal correlations occur because most of the determinants which will permit predictions lie *within* the organism in any given situation, rather than in the immediate objective situation.

This method of explaining by means of inferred factors has been used by Hull's group as well as by Gestaltists and others. Hullian thought,

however, has not taken sufficiently into account the enormous complexity of these internal processes, their dynamic-organizational character, nor their frequent step-wise changes which result from the gradual, cumulative effects in some other parts of such complex systems. Evidence for these points comes from a number of as-yet unpublished studies on the "non-continuity phenomenon" in learning, and from published studies on the "non-continuity phenomenon" in learning, and from published experiments by Saldanha and Bitterman, Witkin, Köhler, and others.

We constantly have the risk, when we try to use this "thinking" type of methodology, that we will overlook some crucial factors, base our principles on too miniature an area of research, or somehow over-rate the amount of real "explaining" that we have done. Thus, somewhat as Brunswik claims, probably Gestalt psychology has not made a sufficient attempt to explain the fidelity or veridicality of cognitive processes. On the other hand, it is not adequate to "explain" veridicality merely as Brunswik tends to do—by citing evidence that considerable fidelity exists. The things that account for fidelity must be understood, probably, in terms of a field theory developed, not only in a sense clearly defended by Murphy, but also in two related senses seen in Lewin and in Köhler and Tolman.

The type of approach earlier advocated by Brunswik cannot be a substitute for such more complex thinking. But, it should be a part of our method—as in his experiment—as a means of guarding against "brilliant absurd errors".

## THE BOUNDARIES BETWEEN THE INDIVIDUAL AND HIS WORLD

BY

GARDNER MURPHY (*Menninger Foundation, Topeka, Kansas*)

The boundaries between person and world may be viewed in terms of physical, biological, psychological, and social science. All four suggest that boundaries call for clarification.

From Democritus to Schrödinger the physical view suggests that what is inside man is essentially like what is outside; he is a bit of the cosmos, duplicating it in substance, frequently also in structure. Cosmic environment permits momentary isolation of small portions of itself. L. J. Henderson has shown that in vital processes like respiration it is meaningless to define where the boundary lies.

Biological research nevertheless suggests that within an "open system", effective principles (e.g., homeostasis) may differ from those generally obtaining outside. We may avoid dualism of substance, yet provide dualism of function (C. D. Broad). Life exists precisely because within a protected area unique processes go on. It seems important to ascertain how far Henderson's conception of the *gentle gradient* holds good, how far the *sharp gradient* of homeostatic isolation prevails. Some philosophers (T. H. Huxley, B. Russell) assume paradoxically that while man is made of cosmic stuff, his values are irreconcilable with cosmic processes.

From the viewpoint of psychology, the gradient is ordinarily steep. Thus in cognition the person is conceived to apprehend what is fully external to himself. Yet psychology offers three other approaches. The Gestalt conception of inner structure reveals a parallel with the sharp gradient of homeostasis. Isomorphism represents inner structure as mirroring outer structure, not because man is (as with Hecht) at the mercy of the environment, but because his own dynamic is attuned to it.

The second conception presupposes formal independence of the environment, combined with absolute dependence through give-and-take, the tennis game of acts and signals, the world of cybernetics, of Brunswik's navigation through a sea of uncertainties, Leeper's complex contacts with heterogeneous reality, Sears' dyadic functions—a world whose reality is *reciprocity*, not the events-within-the-person which our self-reliant grandparents knew.

The third conception goes even further. In the life-space of Lewin there is no fixed division of inner and outer, and in some types of field theory a unified world of person and environment yields a gradient as gentle as that of Henderson, with implications which encounter resistance proportional perhaps to our culturally ingrained conviction that there *must* be something utterly our own.

If we view this problem not "from outside", but from the standpoint of the experiencing person, some fragmentary evidence suggests several *different kinds* of gradients between self and non-self: evidence regarding the child's slow development of awareness of self and of others; evidence from psychiatry and anthropology regarding forms of experience ranging from sharply defined, pathologically distinct awareness of self to the opposite pole of depersonalization in which selfhood is lost. The psychology of India has explored such experiences, both empirically and conceptually. No psychology aspiring to completeness can ignore them.

Another challenge is offered by experiments in parapsychology, in which distant randomized material has now been shown in a dozen laboratories

to be accessible to cognition, in confirmation of hypotheses regarding variables likely to soften psychological barriers between perceiver and object. In S. G. Soal's work, for example, the minds of three persons interact in a unitary field.

Finally, social science emphasizes interdependence, "membership character" of person in group, making it sometimes operationally impossible to chop apart interpersonal totalities.

Perhaps we might conclude that some aspects of persons are relatively sharply bounded, others the reverse. Paradoxically we give heroic stature both to the supreme Whitman-like "individualist" and to him, like Gandhi, who is selfless in devotion to a social goal. In an era in which humanity can hardly expect to survive without fuller knowledge of the biosocial value of individualistic and social impulses, research is needed into the sources both of man's need to isolate himself and of his need to soften his individuality in the presences of his fellow men and of the sea and the stars; we need to understand how his culturally molded selfhood sometimes leads him to make the gradient much *less* steep, sometimes much *steeper*, than it is.

# SYMPOSIUM

## RECENT DEVELOPMENTS IN SENSORY PSYCHOLOGY

## LES PROGRÈS RÉCENTS DE LA PSYCHOLOGIE SENSORIELLE

### *Chairman — Président*

LORRIN A. RIGGS (*Brown*)

### *Discussion*

FORREST LEE DIMMICK (*New London*), HENRI PIÉRON (*Paris*),  
MATSUSABURO YOKOYAMA (*Tokyo*), KOITI MOTOKAWA (*Sendai*),  
WALTER R. MILES (*Yale*)

### *Papers — Communications*

## LUMINOSITY CURVES FOR NORMAL AND DICHROMATIC SUBJECTS

BY

C. H. GRAHAM and YUN HSIA (*Columbia University*)

In 1952 we published data on the spectral sensitivity of cones in normal subjects. The findings indicated that the luminosity function shows a maximum of sensitivity near 550  $m\mu$ , with humps in the blue and the red. This finding is in line with some of the results obtained by other investigators. The data were obtained by threshold measurements.

We have recently examined a group of dichromats, consisting of 5 protanopes and 5 deutanopes. The classification of subjects was based on examination by the Stilling and Ishihara tests, determinations of the Rayleigh equation on the Shlaer anomaloscope, and measurement of neutral points on a modified Helmholtz color mixer.

The protanopes show the expected lowering of luminosity on the long wavelength end of the spectrum. As a result, the hump at 610  $m\mu$  is practically obliterated. Throughout the rest of the spectral range the sensitivity of the protanope is about the same as that for the normal.

In the case of the deutanopes, it can be shown that the luminosity

curve exhibits reduced sensitivities in the wavelength range between 530 m $\mu$  and about 415 m $\mu$ .

It may be hypothesized that the cone luminosity curve is due to a summation of effects in different sets of color receptors, each set having its own form of absorption curve. In such a context, the loss of sensitivity shown by the protanope in the red and by the deuteranope in the blue-green may be taken to indicate functional deficits in associated receptors. Computations of fundamental primary curves, based on this notion, are considered.

This research was supported by a contract between the Office of Naval Research and Columbia University.

### DOES THE CAT HAVE COLOR VISION?

BY

RALPH GUNTER (*London*)

The evidence recently adduced by Chang for the cat suggests strongly that this animal's visual system functions as a triple conducting pathway. Furthermore, by using monochromatic spectral stimuli, Chang was able to produce—at the cortical level—selective potentiation of electrical responses.

By the use of the micro-electrode technique, and adapting the cat retina selectively to lights of different wave-lengths, Granit was able to isolate three narrow response curves (modulators) having their maxima of sensitivity respectively in the red-yellow (580—600 m $\mu$ ), the blue-violet (440—460 m $\mu$ ) and the green (500—540 m $\mu$ ) region of the spectrum.

In order to determine to what extent this peripheral colour-discriminating mechanism is correlated with an ability to make use of this information, four light-adapted cats were trained in a Y-shaped discrimination box to respond differentially between filtered lights of eight spectral regions, isolated by Ilford spectrum filters (601—608).

To ensure that the animals' discrimination between the spectral stimuli be on the basis of differences in wave-length, it is essential that the stimuli presented be of equal brightness to the animals. Any discrimination between spectral stimuli thus matched in brightness may then be taken as evidence of "colour vision". Thus, it was essential first to determine the spectral sensitivity of the light-adapted animals.

When pairs of spectral stimuli to which the animals had given an average response of brightness equality were presented, none of the animals was able to discriminate between them after 950 trials. An

additional 500 trials to three selected spectral regions also provided no evidence of learning. A discrimination on the basis of brightness had been learned by the same animals after approximately 200 trials in previous experiments.

This absence of colour discrimination had also been suggested by the fact that in the previous spectral sensitivity determinations with these animals, a level of intensity of the spectral stimulus was found for each animal which it confused with an opposing white light stimulus even after 3000 training trials.

As a control experiment, four new animals which had never been trained in brightness discrimination were introduced into the stimulus situation in order to determine whether these animals could be trained to discriminate between three pairs of spectral lights of approximately the same intensity levels to which the experimental group was unable to respond differentially. 950 trials were given to each animal and no learning occurred throughout the experiments.

When the learning ability of these animals was tested by training them to discriminate between a "coloured" and a white light with large difference in intensity between them, all animals learned this discrimination in an average of 200 trials.

Light-adaptation was maintained by having high over-all illumination, and by means of eight 100 W lamps mounted all along the length over the experimental box. The light stimuli were surrounded by white material of high reflecting power and the illumination of the stimulus panel was kept constant at 200 ft. c.

Since the intense light-adaptation produced a constriction of the animals' pupil to a narrow slit, control experiments were run, keeping the animals' pupil open with homatropine. These experiments were done on four cats (two of each group) to the blue, green and red parts of the spectrum. No learning was evident after 900 trials.

Whenever the animal responded to one of the two simultaneously presented spectral stimuli more than 13 times in 20 trials, the intensity of the opposing spectral stimulus was decreased. When that was done, the response fell back to random frequency.

On the basis of these results it is suggested that the peripheral "colour vision" mechanism postulated to exist in the cat's retina does not seem linked to a corresponding ability to utilize the information received from the peripheral sense organ. The results obtained in this investigation might be taken to indicate an absence of colour discrimination in this species under the conditions of the present experiment.



## BRAIN CONTROL OF THE SENSE ORGANS

BY

RAGNAR GRANIT (*Stockholm*)

The old assumption that the brain may control peripheral sense organs by specific fibres is to-day receiving experimental attention. Granit and Kaada (1953) proved that various parts of both brain stem and cortex can excite and inhibit the discharge from the muscle spindle by way of Leksell's (1945) efferent gamma fibres, studied in the United States by Kuffler and Hunt (1952). Examples of inhibition and excitation of the muscle spindles from the brain will be given in slides.

Here then is a sense organ that the brain can set to different levels of sensitivity, as expressed by impulse frequency, and we can study the principles by which this feedback loop is run. All work on it shows it to be meant for close purposeful collaboration with the motor fibres of the muscle. This statement will be illustrated by slides.

The muscle spindles are end organs of the slowly adapting type and have long been known to be of importance for muscle tonus. Since their frequency can be varied from the brain, the latter also obtains control over tonus by the loop through the spindles, an arrangement of considerable theoretical interest.

Part of this interest derives from the fact that the muscle spindles are strongly influenced by the general facilitatory and inhibitory centres in the reticular substance, as described by Magoun and his collaborators (Magoun, 1952).

The reticular excitatory centres receive an unspecific input of afferent fibres from most sense organs, including eye and ear, and their stimulation produces in the cortex the awakening or arousal reaction, well known in electroencephalographic terms. Hence spindle activation, apart from being beautifully organized to collaborate with muscle action, is also involved in the general arousal reaction.

This led me to examine the retina for which centrifugal fibres have been described by Cajal and Dogiel long ago. Micro-electrode records from single ganglion cells were used and it proved possible to excite these cells from the reticular activating centre in the brain stem. This will be illustrated by slides. The results are definite, as far as they go, but it is nevertheless possible to attribute part of the effects to indirect arousal through vascular effects, because Ingvar (1954) has shown that the retina, observed in a microscope, responds to reticular stimulation with vascular dilatation.

This problem is complex. When the reticular activating centre is stimulated, centripetal optic nerve fibres are not excited, but it is possible to "drive" retinal ganglion cells from their end stations in the pretectum and the superior colliculus and in this case the most general after-effect is a facilitation of considerable order of magnitude, obtained only as long as the centripetal fibre isolated in the retina is actually driven. The effect diminishes if the electrode is moved up or down so as not to excite centripetal fibres antidromically.

Sometimes the retinal spike is found to be inhibited. Like excitation from the brain, inhibition has also two aspects: it is obtained after "driving", but also from places in which no "driving" has occurred. These effects will be illustrated by slides.

Finally Hagbarth and Kerr (1954) have shown that the somatic afferent discharge, inasmuch as it is relayed within the spinal cord can be influenced from various places in the brain.

All this goes to show that the idea of central feedback control of sense organs now has become a programme of research deserving close attention.

It is impossible here to refer to all the individual papers concerned. I have recently summarized the present state of this problem elsewhere (Granit, 1954, with full references).

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#### THE EXCITATION OF VISUAL RECEPTORS, AND THE PATTERNS OF NERVOUS ACTIVITY IN THE EYE

BY

H. K. HARTLINE (*Rockefeller Institute, New York*)

An analysis of the mechanism of vision may be approached by a consideration of how light initiates nervous activity in the receptor elements of the eye. The first steps in this process—the photochemical

reactions that occur upon the absorption of light in the sensory element, and the other biochemical mechanisms that support the photochemical system—are beginning to be well understood. Little is known, however, of the process that intervenes between the initial photochemical reaction and the generation of nervous activity in the optic pathway. In some lower animals it has been possible to study, by electrophysiological methods, the activity of individual visual receptor units, and this has been useful in investigating the mechanism of receptor excitation. In *Limulus*, an individual element (ommatidium) of the compound eye can be isolated by dissection, and a comparison can be made between the discharge of nerve impulses in the strand of optic nerve fibres arising from it and the slow "retinal" action potential generated by the cluster of cells that comprise its sensory structure. Electrical potential changes have also been recorded by a micropipette electrode thrust into one of the cells (the eccentric cell) of an ommatidium. A slow electrical depolarization of the cell has been observed in response to light, and with it a succession of rapid spike-like action potentials, each one synchronous with the discharge of a nerve impulse in the cell's axone. It seems likely that this depolarization, whose external manifestation is the retinal action potential, is an intimate part of the mechanism whereby nerve impulses are generated.

The ommatidia of the *Limulus* eye do not function independently of one another; each one exerts an inhibitory action on those adjacent to it, to a degree dependent on its level of activity. This action is mediated through a non-ganglionic plexus of nerve fibers that lies just behind the layer of receptor elements. Its effect is to enhance contrast in the visual image. More complex integration of receptor information can be observed where there is more elaborate neural organization. Thus in the optic lobe of the central ganglion in *Limulus*, "off responses" are generated when optic nerve activity stops or diminishes. In the complex vertebrate retina diverse patterns of response, suitable for signalling sudden changes of illumination and movements of the retinal image, are obtained from the retinal ganglion cells. The process of integration of nervous activity begins at levels of the visual pathway very close to the receptors from which the activity originates.

SUBJECTIVE SCALES FOR THE MEASUREMENT OF  
BRIGHTNESS AND LOUDNESS

BY

S. S. STEVENS (*Harvard*)

Brightness and loudness are subjective attributes of visual and auditory sensation. As previous experiments have shown <sup>1</sup>, these attributes are not simple linear functions of stimulus intensity. Nor are they proportional to the logarithm of the intensity, as was first proposed by Fechner.

We have undertaken to study the relation of brightness and loudness to stimulus energy by means of three general methods: (1) *equisection*, in which the subject adjusts one or more stimuli to make the apparent distance between successive stimuli appear equal, (2) *fractionation*, in which the subject adjusts one stimulus until it appears to stand in a certain relation (e.g., one-half) to a standard stimulus, and (3) *numerical estimation*, in which the subject judges the apparent magnitude of a variable stimulus. The scale on which he judges is fixed by assigning some arbitrary value to a standard stimulus. For example, if the loudness produced by a tone of 80 db is called 100, the subject's task is to estimate, on a scale from 0 to 100, the loudness produced by a tone of, say, 60 db. Although these three methods show considerable agreement, each of them is subject to systematic errors of one sort or another, and the task before us is to locate these sources of bias and to compensate or eliminate them.

The method of equisection shows a striking *order effect*. The apparent midpoint between two stimuli is higher when the stimuli are presented in ascending order; it is lower when they are presented in descending order. We also observed this same order effect with lifted weights.

The methods of fractionation and numerical estimation give data from which ratio scales for brightness and loudness can be constructed.

There appears to be a remarkable similarity between brightness and loudness when their magnitudes are plotted against a decibel scale of stimulus intensity. Over most of the sensible range of intensities, both attributes are approximately proportional to the cube root of the stimulating energy. This means that, in order to double the subjective magnitude of a light or a sound we must raise the intensity of the stimulus by roughly tenfold (approximately 10 db). Actually, for most subjects

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<sup>1</sup> For example of recent work on loudness see D. W. Robinson, *Acoustica*, 3, 1953, 344—358, and on brightness see R. M. Hanes, *J. Exp. Psychol.*, 39, 1949, 719—728.

brightness seems to grow slightly more rapidly than loudness for a given decibel increase in the stimulus.

Other similarities between brightness and loudness are also evident. A sound is masked by noise much as a light is suppressed by glare, and the well known phenomenon of "auditory recruitment" in which the loudness of a masked sound grows rapidly to its normal loudness as its intensity is raised has its analogue in vision. A light that is just invisible in the presence of a glare grows rapidly to its normal brightness when its intensity is raised to a value equal to or greater than that of the glare itself.

WEDNESDAY, JUNE 9 — MERCREDI LE 9 JUIN

## SYMPOSIUM

### PRESENT STATUS OF FREUDIAN THEORY

#### ÉTAT ACTUEL DU FREUDISME

*Chairman — Président*

E. R. HILGARD (*Stanford*)

*Discussion*

GERALD S. BLUM (*Michigan*), NOËL MAILLOUX (*Montréal*),  
CECILY DE MONCHAUX (*London*)

*Papers — Communications*

### RECURRENT TRENDS IN RELATION TO PSYCHOPATHOLOGY

BY

GREGORY ZILBOORG (*New York*)

Two particular trends in the history of the development of human psychology will be touched upon.

1. The general attitude towards human psychology with the tendency of fusing it with religion and philosophy. Emphasis will then be laid on the fact that it was general medical science which, as a result of clinical contact with human beings, made this major contribution to the business of separating psychology as a science or a scientific discipline and, what is more, a discipline more intimately related to man as a functioning unit and totality rather than to man in his relation to the universe and to God.

2. The history of psychoanalysis reflects rather poignantly the confusion of psychology with philosophy and religion on one hand and with neuro-anatomy and neuro-physiology on the other. The evolution of Freudian theory will then be briefly traced. It will be historical perspective and it will be demonstrated from the purely historical point of view how Freud, not a historian himself, sought light with regard to human

psychology via the first empirical syntheses of the sixteenth century; it was only after Freud established contact, as it were, with that period that he set himself to the arduous and the heroic task of self-analysis.

## RESEARCH POSSIBILITIES IN PSYCHOANALYSIS

BY

LAWRENCE S. KUBIE (*New York*)

In several areas of psychoanalytic theory and technique research is urgently needed:

1. In the transition from biodynamic and psychodynamic levels of integration. This involves specific studies of each of the processes by which the organism makes those interchanges with the environment which are essential for the preservation of its individual integrity and that of the species.

2. A precise study of the processes of free association, the difference between digital and analogic associative processes, on time relationships, content clustering, and content sequences.

3. The development of techniques for the recognition and quantitative evaluation of the influence of concurrent conscious, preconscious and unconscious processes on free associations and on more organized mental activity.

4. The influence of physiological variables on 2, and 3.

5. The further development of the techniques, pharmacological, physiological, and psychological, by which dissociative states can be induced and controlled.

6. Techniques for investigating the influence of conscious on unconscious processes.

7. Out of this constellation of studies would come techniques by which psychoanalytic interpretations can be validated.

8. The development of new methods of recording of speech, intonation, visual expression, mimicry, movement-tensions, etc. This involves the development of unobtrusive techniques for recording under infra-red illumination, at night as well as in the daytime, and with new methods of recording respiratory and other physiological changes.

9. Such basic "pure" research is pre-requisite to an objective evaluation of therapeutic results, as well as all other applied research in institutes for pure research, free of practical responsibilities for either therapy or teaching.

THE CONCEPTUAL STRUCTURE OF FREUDIAN THEORY AS  
RELATED TO EXPERIMENTAL VERIFICATION

BY

WALTER TOMAN (*Harvard*)

Freudian theory is said to be gross in its vocabulary and animistic in its grammar. Yet it covers a wider realm of phenomena, has a greater number of followers, a larger bibliography, etc., than any other personality theory. In addition it says and suggests more, and it has influenced more other personality theories than did any of the others.

Freudian theory has two main portions: libido theory and structure theory. These, however, are only two aspects of one theory. Libido theory comprehends three to seven developmental stages (depending on how specific one wants to be), and structure theory three systems of psychological forces.

Why are there three and no more systems? Why are there different stages? Why do they occur in a certain sequence? In order to answer such questions—and a true theory should attempt to account for all its peculiarities—we will start from more basic assumptions than those about stages and systems, and try to be more precise and explicit.

These basic assumptions are: Libido is the energy to search for and consume sources of pleasure. Its quantity is constant unless reduced by alarm or anxiety. Consummation of pleasure, and only consummation of pleasure, results in the recruitment of data from the world, or learning, or cathexis. (In fact, recruitment of data is its main measure.) Through recruitment of data there are formed: sources of pleasure as well as expectations and motives to reach consummation of these sources of pleasure. Among the data recruited there are some (i.e. behavior) that can elicit the rest of the data. To the extent that the data recruited include those that can elicit the rest expectations turn into motives. Recruitment of data facilitates search for and consummation of pleasure as well as the arrest and return to zero of increasing alarm or anxiety.

From these assumptions relationships among motivation, consummation of pleasure and alarm, the function of alarm, the increase of variability of motive intensity, the concept of aggression, the relationship between alarm and aggression, the defence mechanisms and their relation to the individual's reality concept, and finally even the major peculiarities of psychic structure and psychosexual stages seem to be derivable, at least in principle. Furthermore the constitutionally given speed of recruitment of data



as well as lack, to any degree, of supply of pleasure by the environment, both with all their implications, are shown to be capable of accounting for important overall individual differences.

A theory brought into some such form consists of nothing but verifiable propositions. What is even more: if theory construction has been consistent, the test of any proposition will have bearing on the entire theory, and under such a premise experimental and observational verification is really meaningful and worthwhile. In principle, one single experimental or observational evidence may suffice to change the entire theory. On the other hand, the better constructed a theory is, the more trivial, in a sense, will it tend to be, and the less likely is such an evidence to occur.

## SYMPOSIUM

### INDIVIDUAL DIFFERENCES IN ABILITIES AND THEIR IMPLICATIONS FOR NATIONAL POLICY

### DIFFÉRENCES INDIVIDUELLES D'HABILETÉ ET LEUR INFLUENCE SUR LA POLITIQUE NATIONALE

*Chairman — Président*

DAEL WOLFLE (*Washington*)

*Discussion*

JOHN E. ANDERSON (*Minnesota*), ALASTAIR HERON (*London*),  
OTTO KLINEBERG (*Paris*), M. REUCHLIN (*Paris*)

*Papers — Communications*

### INDIVIDUAL DIFFERENCES IN ABILITY AND THEIR IMPLICATIONS FOR NATIONAL POLICY IN SOUTH AFRICA

BY

S. BIESHEUVEL (*Johannesburg*)

The study of individual differences in South Africa is greatly influenced by the social structure of the country. Bantu-speaking Africans, who constitute about 68 % of the population, are largely restricted to unskilled and semi-skilled work, though opportunities to proceed beyond these levels in the service of their own ethnic group are increasing. These include professional and administrative work.

As the African population is not clearly stratified according to education, socio-economic status or caste, employers are anxious to make use of aptitude tests to determine the abilities of workseekers. In order to make testing universally applicable in Africa, regardless of differences in language, education and acculturation, a number of performance tests was constructed suitable for mass administration by means of a silent

motion picture. This battery aims at the classification of African labour into non-mechanical, mechanical constructive, mechanical operative and supervisory groups. For the latter, additional tests of the leaderless group type are required.

The tests were first validated on the gold mines, where they reduced wastage among supervisory trainees from 17 % to 3 %. Validity coefficients for this category ranged from .73 to .51 (median .65) with training results, and from .75 to .26 (median .44) with job efficiency. For winch-drivers and non-mechanical categories the range was from .68 to .53. Similar validity results have been obtained in a number of other industries. As the African labour force on the mines, numbering about 300,000 persons, is almost entirely migratory, the average tour of duty being about nine months, the tests have been found useful as a manning device, to supply the correct proportions of labour of a given quality to meet various operational requirements. Factor analysis of the tests showed that they measured a general factor defined as "practical general intelligence".

The effects on test performance of age, sex, education and previous employment are being determined in order that the tests may be used at labour bureaux and for the vocational guidance of African children.

Because of its mode of administration and content, the test is also useful for research into the basic intellectual potentialities of Africans. The control of environmental influences in representative samples of whites and blacks is virtually impossible in Africa, and interracial comparisons are therefore impracticable. It is in any case more realistic to determine the increase in intellectual efficiency that can be expected under African climatic conditions, from improvements in nutrition, health, and literacy, from the availability of secondary and higher education and from changes in family circumstances following the adoption of Western cultural habits. Two investigations, bearing on the effects of deficiency diseases and malaria respectively are being planned.

Research is also necessary into African peoples' capacity for sustained and volitional effort, in which conceivably both ethnic and cultural factors may be operative. It is important that at a time when policies with far reaching consequences for the future of Africa are being developed, the facts on these fundamental questions should be available.

INTELLECTUAL ABILITIES AND SCHOOLING AS  
A PSYCHOLOGICAL AND SOCIAL ISSUE

BY

VIDKUNN COUCHERON JARL (*Oslo*)

If the primary purpose of national policies is to preserve statu quo, applied psychology may, as heretofore, be largely confined to the adjustment of individuals with their differing abilities. But if our aim be the optimum development, productivity and satisfaction of all members of the society, applied social science must be equally concerned with the origins of individual differences and with the long-term conditions for achieving these goals. Then societies, as well as individuals, become the objects of adjustment for applied psychologists.

On the assumption that formal education is an important factor in the development of individuals and of their communities, a series of investigations has been undertaken in Norway in order to ascertain the present situation regarding amount of schooling, to find what other social factors are associated with educational differences, and to study the relationships between schooling and intellectual abilities.

Analyses of both urban and rural communities show large variations in degree of education. These variations appear to be primarily associated with the occupational structure of the communities, thus confirming a recent study of American cities by *Gillen*.

A similar relationship is found when the communities are compared with respect to the reasoning test ability of their military recruits. The data seem to indicate, however, that educational variations can not alone account for the differences in test performance. But they do show that rural boys, and particularly those coming from the least industrialized communities, get less education than their urban age-mates, even if their measured ability as adults places them in the upper quarter of their age group.

If education produces better people for society by improving their knowledge, work methods and attitudes, society is not taking full advantage of this fact. And if schooling beyond the primary grades is furthering intellectual development in general, this neglect is even more far reaching.

Those recruits who have changed domicile in the course of their developing years have higher average test scores than the non-migrants. This aspect is being further studied. But since both selection and training factors may be at work, cross-sectional studies such as ours are difficult

to interpret. They are therefore being supplemented by a longitudinal study of 7,700 pupils who were tested last year in the seventh grade. At the age of 19, the boys will be given the same tests in connection with their classification for military service. Thus we hope to throw light on the neglected problem of the differential effect of schooling on intellectual development, and at the same time to ascertain what selective factors are influencing the attainment of advanced schooling in Norway.

## SOME HISTORICAL AND EXPERIMENTAL REMARKS ON SELECTION PROBLEMS IN SPAIN

BY

MARIANO YELA (*Madrid*)

Individual differences and social structure are two sides of a single problem. This problem could be termed "selection". I mean by selection the biosocial process by which the members of a society become differentiated into its various tasks.

The problem for us is to consider the part that individual differences in ability play in this process: how these differences can be discovered and utilized; how this utilization can modify society; how the social structure itself modifies the individual differences in ability and their distribution.

The work that has been done in Spain in this field is summarized.

First, two anecdotes are briefly presented: a historical event and a historical man. The event shows the influence of selection in national achievement as revealed by the change that took place in Spanish society by the end of the XVth century. The man is Huarte de San Juan, the first (XVIth century) who treated systematically the problems of guidance and selection.

As to the present, a description of the organization and research of selection problems as they have been studied in Spain is given and some experimental data concerning the criterion problem, school and industrial selection and group differences in ability are presented.

The report ends with some general remarks on the institutional and research aspects of selection problems.

## INDIVIDUAL DIFFERENCES IN ABILITY AND SOCIAL RECONSTRUCTION

BY

A. H. EL-KOUSSY (*Cairo*)

Our national policy is consciously directed towards the increase in productivity of both human and natural resources which, in the past, were largely wasted due to a large combination of interconnected external and internal forces. I do not intend to give an account of the whole picture but I am going to mention a few salient features:

- a) The national income per individual is indeed too small to meet the fundamental human needs.
- b) The increase in the population is far quicker than the increase in cultivable land which is at present the main source of wealth.
- c) Until recently the socio-economic structure was almost feudalistic: over 95 % of the land was owned by less than 5 % of the people.
- d) In the past also, education was planned in such a way that only the upper and middle classes could benefit by it and the poor—which is the great bulk of the population—were barred from it.
- e) The inevitable solution rests in the increase of cultivable land, industrialization, the extension of education, and the application of social justice.

The new education act of 1953 based a good portion of its policy on findings of local research on human ability. It leads to more homogeneity in age and ability and gives ability priority over any other consideration in determining the right to follow one course of education or another.

What is taking place in this respect in Egypt has taken place in England through the 1944 Act, in France through the suggestions of the Langevin Committee and in other parts of the world.

There are a number of researches on human ability which may throw some light on environmental influences. The Ballard group test for juniors which was previously applied in England and in Belgium was translated and adapted to Egyptian school children in 1929 in a research conducted at the time by Claparède. The test became almost totally different from the original. The results of this new test, known as Kabbani's, could not at all then be compared to those of the original applied in London or the adaptation applied in Belgium.

About four years ago, Mr. Scott conducted an extensive experiment on the school children of the North Sudan using Kabbani's adaptation of the Ballard test and a large number of other tests. Scott found that two-thirds

of the Kabbani's questions were non-discriminating. Such a fact precludes all possibilities of comparison.

Scott came to the conclusion that translations and adaptations of foreign verbal group tests are invalid and unreliable. Pictorial tests are also useless.

Dr. Fahmy travelled further south, not with the intention of applying ability tests in the traditional way, but with the purpose of making a preliminary exploration. He came back with some tentative conclusions about his subjects:

- 1) They likely have some sort of ability which is not easy to fathom by our own traditional methods.
- 2) Their concepts are a function of their own environment.
- 3) Agreement with Biesheuvel's statement that a culture-free intelligence test is really a contradiction in term.

All that I've just mentioned about this kind of research is incomplete and, yet, illuminating in showing how careful we should be when working with people from different environments and with different emotional tendencies, attitudes, habits and smills. Psychologists must either stop comparing different environmental, sex, and race groups or find some criteria based on general universal principles. Some may say: "But, that is impossible". My answer would then be that, in such a case, comparisons are also impossible to make.

In any case we are in great need of going much deeper into the nature of abilities and the extent of their amenability to training. This, I feel, would help elucidate a number of questions concerning social order or national policy.

Can we say at this stage that there are differences between the sexes, races, and nations or between the sub-groups which justify the existing explicit denial of equal opportunity to all?

In the first place there is the belief amongst the masses that people are born equal and therefore they must be given equal opportunity for education, for work and for acquisitions, achievements and status in society.

The question of differences brings in a very long debated question, i.e. the freedom of the individual versus the control by the state.

The answer to such a question is that we must have a public which is enlightened and a government which is sympathetic, strong, conscientious and enlightening.

The only real social justice should work towards the demolishing of *all barriers and should* if possible be based on one thing—ability and its constructive concomitants.

## SYMPOSIUM

### RECENT ADVANCES IN CONDITIONING

### APPORTS RÉCENTS DANS LE CONDITIONNEMENT

*Chairman — Président*

DONALD G. MARQUIS (*Michigan*)

*Discussion*

PAUL FRAISSE (*Sorbonne*), GREGORY RAZRAN (*Queen's, Brooklyn*),  
E. L. KELLY (*Michigan*)

*Papers — Communications*

### LA NATURE ET LA FORMATION DES PROPRIÉTÉS ET DES PROCESSUS PSYCHQUES DE L'HOMME

PAR

A. LÉONTIEV (*Université de Moscou*)

Une étude qui se borne à la constatation des particularités psychiques et de leurs corrélations ne permet pas d'approfondir notre connaissance de la nature même de ces propriétés.

Ce problème ne peut être résolu que par une recherche sur les mécanismes réels dont le fonctionnement est la manifestation de telle ou telle particularité psychique d'un individu.

Nous savons que certaines facultés de l'homme, même aussi universelles et aussi simples que celle de la perception des objets par exemple, se forment ontogénétiquement.

Ceci se fait au moyen de l'établissement de réflexes conditionnels qui, en se fixant, forment des systèmes relativement stables de liaisons cérébrales. La formation et les changements de pareils systèmes peuvent être étudiés avec succès en laboratoire.

Ainsi, par exemple, l'ouvrage de A. Léontiev et A. Zaporogietz (1945) décrit en détail la formation de nouveaux rapports tacto-visuels, dans un cas de stéréognosie dû à une blessure à la main et à une greffe des tissus.



E. Sokolov a réussi à déplacer la localisation acoustique d'un objet produisant un son par la formation de rapports audio-visuels (1952).

La formation des particularités et des propriétés psychiques spéciales (qui ne se trouvent pas chez tous les individus) est un cas séparé intéressant. Le trait caractéristique de ces particularités consiste en ce que la formation des systèmes de réflexes qu'elles représentent ne se passe que dans des conditions spéciales. Ces particularités ne se forment pas toujours et peuvent avoir des structures différentes chez les individus. La formation primaire de ces systèmes pendant le développement ontogénétique étant incontrôlable et cachée, leur fonctionnement nous apparaît comme la manifestation de particularités spéciales héréditaires.

L'étude de la vocalisation des perceptions auditives, qui est un exemple marquant de faculté spéciale, a été menée par l'auteur conjointement avec J. Hippenreiter (1954). Les sujets qui donnaient des résultats négatifs aux premières expériences purent, après qu'on eût élaboré au laboratoire le système nécessaire de liaisons réflexives, résoudre des problèmes de vocalisation inaccessibles pour eux auparavant.

Le problème de la nature des particularités et des processus intellectuels complexes de l'homme présente un grand intérêt théorique et pratique.

En 1930, l'auteur avança une hypothèse suivant laquelle les processus intellectuels internes se forment par la transformation des opérations réalisées d'abord à l'extérieur. Cette hypothèse nous apparaît dans une lumière nouvelle grâce aux recherches menées ces dernières années par nos collaborateurs P. Galpérine, V. Davydov et I. Morosova. Ces savants ont étudié chez les écoliers la formation et le fonctionnement de processus intellectuels tels que le calcul mental et les opérations impliquant des catégories géométriques abstraites. Ainsi que l'ont démontré ces études, la formation de ces processus commence par l'assimilation de l'action avec les objets, puis cette assimilation passe dans le langage des sujets et, enfin, elle revêt un caractère abstrait, se généralise et se transforme en processus interne qui subit enfin une « réduction » spécifique et s'affermir comme stéréotype dynamique.

Après ce dernier changement le processus perd les traits caractéristiques d'une action externe et acquiert la forme d'un acte psychique interne. Il est déjà difficile alors de reconnaître dans ce processus le résultat d'actions extérieures, apprises par l'enfant ; il nous apparaît plutôt comme la manifestation de la faculté de l'esprit de passer immédiatement des données d'un problème à sa solution.

Des recherches sur l'« insight », portant sur l'incapacité d'accomplir ces actes, nous prouvent que nous sommes en présence d'un système de

fonctionnement formé irrégulièrement qu'il est possible de réformer.

Pour ce faire, comme l'a montré l'expérience de L. Slavina, il faut trouver les chaînons manquants du système et replacer l'écolier dans la situation où ces chaînons s'établissent dans leur forme primitive (comme opération extérieure).

Pour conclure, les propriétés psychiques de l'homme ne sont pas la manifestation de « forces » spécifiques dont la présence ou l'absence ne peut être que constatée, mais le résultat d'un processus ontogénétique compliqué. La connaissance des lois qui régissent ce processus nous permet de le commander rationnellement.

## THE HIGHER NERVOUS ACTIVITY AND THE PROBLEM OF PERCEPTION

BY

E. N. SOKOLOV (*Moscow University*)

Considering perception as a reflex process, we have studied the role of the *orienting reflex* in human perception. We have made complex measurements of the light-sensitivity of the eye, the electrical activity of the visual cortex, and of the arm muscles, as well as of the skin-galvanic and vasomotor responses to indifferent (nonsignal) and conditioned (signal) visual, auditory, tactile and proprioceptive stimuli. The data indicate that these responses may be components of the orienting reflex, a complex reaction of the whole organism which occurs when the stimulus begins or changes, and which provides the best conditions for the perception of the stimulus. Apparently, the orienting reflex has two forms: a *phasic form*, in which the responses soon return to a constant level, and a *tonic form*, which results in lasting changes in the sensory threshold.

When the same stimulus is repeatedly applied, orienting reactions to it become extinguished, and this extinction is generalized to similar stimuli. At the same time there is a positive induction of the orienting reactions to other stimuli which were not applied during the experiment.

Orienting reactions to a specific stimulus which have become extinguished reappear when the stimulus becomes a signal for response-activity by the organism. The reflex appears in a generalized form when it is revived; orienting reactions occur when applied stimuli are somewhat similar to the conditioned one, while, at the same time, there is an inductive inhibition of orienting reactions to dissimilar stimuli.

During the formation of the conditioned response, there are orienting

reactions to all changes in the stimulus. However, these reactions gradually extinguish, though all their components are not inhibited simultaneously. Thus, there is a narrowing of the range of stimuli which will elicit orientation reactions as a result of the concentration of excitation on the systems which are involved in the perception of the conditioned stimulus.

If a difficult discrimination is presented, however, the orienting reactions reappear. A special case of this occurs when the conditioned stimuli used are near the absolute sensory threshold, when particularly strong and lasting orienting reactions develop. Conditions in which it is difficult to make a discrimination also cause an increase in the latency of the conditioned motor response. This inhibition indicates that the response is delayed until the properties of the stimulus are clarified by orienting reactions.

When orienting reactions are inhibited, there is a decrease in the sensitivity of the "analyser system"; when they recover, the system becomes more sensitive. In the case of a difficult discrimination, the strengthening of the orienting reactions is connected with the increase in reactivity of the "analyser system" affected by the conditioned stimulus, as well as other "analysing systems". When response-activity occurs because the "analysing system" perceives the conditioned stimulus, the excitation which develops exerts its influence by means of the two-way passage of excitation along the conditioned reflex arc.

## SWITCHING IN THE HIGHER NERVOUS ACTIVITY

BY

E. A. ASRATYAN (U.S.S.R.)

Pavlov considered conditioned reflexes to be the central physiological phenomenon in the activity of the large cerebral hemispheres, and changeability to be one of the principal characteristics of conditioned reflexes. Thus, the highly developed organism is capable of adapting quickly and efficiently to the constantly changing conditions of the environment. Switching in CR activity (the same stimulus causing different responses under different conditions) is also an example of CR changeability.

The author distinguishes between two types of switching: (1) that due to regularities of innate forms of activity in the central nervous system, and (2) elaborated switching. An example of the first type is the case in

which the signal stimulus causes a CR of a particular kind because the centre for the UCR at that time has a higher level of excitation or excitability than the other centres. The second type of switching was found during experiments carried out by the author and his colleagues. It differs in that the signal stimulus gives rise to a CR of one type or of another—not because of the regularities of innate forms of nervous activity but because this property of signal stimulation is specially developed by specific training. This CS possesses simultaneously two different signal meanings, each of which can be dominant, depending on the circumstances.

For example, let two experimenters work with the same dog in the same experimental room, but at different times of the day. If one of them presents a bell with food and the other the same bell with an electric shock to the animal's paw, when the experiment is carried out by the first experimenter, the bell causes salivation, while when it is conducted by the second the same stimulus results in foot-withdrawal. Similar results are obtained when the experiments are carried out by the same individual in different rooms, or in the same room but with the experiments given in a different order.

More complicated forms of switching are also possible. If a dog is taught to have different CRs to two different stimuli (e.g. salivation to an auditory stimulus and foot-withdrawal to a tactile one) in one experimental room, it is possible to train him to salivate to the tactile stimulus and withdraw his paw on presentation of the auditory stimulus when he is in a different room. We can also teach him to react positively to 120 metronome beats per minute and negatively to 60 in one set of circumstances, and to react positively to 60 beats per minute and negatively to 120 in another.

From the above evidence, it appears that the second type of switching is a product of training; that is, it has some characteristics of a conditioned reflex. The agent of switching is a specific conditioned stimulus which produces in the large cerebral hemispheres a definite CR functional background of a tonic character, a definite dynamic structure of open and closed CR routes, and thus it predetermines the character of CR activity.

Switching cannot be reduced to a complex CR, to a CR to the situation, or to a conditioned inhibition. Though it possesses some features in common with each of these, it is different from them, and is a peculiar phenomenon in CR activity. Regularities of switching promise to throw light on the mechanisms of many complicated psychological phenomena.

CONDITIONED AUTONOMIC RESPONSES IN THE  
EXPERIMENTAL STUDY OF ANXIETY

BY

JOHN I. LACEY (*Fels Research Institute*)

It is well known that human conditioning is markedly affected by the subjects' cognitions of and attitudes towards the experiment. These results raise the possibility that conditioning may prove to be a sensitive tool for analyzing the differences between conscious and unconscious cognitive and affective processes.

While apparently being tested for his ability to coordinate (a) the mental task of rapidly producing chained associations to each of a series of stimulus words with (b) the motor task of tapping a telegraph key at a constant and even rate, the subject was actually being conditioned to one of the words in the list. Electric shock to the arm was used as the reinforcement. The subject believed that the shock was used to periodically stimulate the muscles he used in tapping.

The words "cow" and "paper" each appeared 6 times in a list of 40 stimulus words. The other words either had obvious rural connotations (e.g. farmer, harvest), or were not at all related to rural objects (e.g. copper, blue). The period of association and tapping was 15 seconds each. Reinforcements were administered, when called for, at the end of these 15-second intervals. Half the subjects were reinforced after "cow", half after "paper". Half the subjects were unaware of this relationship, as determined by intensive interview subsequent to the conditioning session; half were told in advance after which word they would be shocked. There were 10 subjects in each group. An extinction session immediately followed the conditioning session.

The responses of heart rate, skin resistance, and digital blood flow were measured in the 15-second intervals. Heart rate proved to be the clearest indicator of conditioning and generalization.

'Unconscious' conditioning and generalization was clearly demonstrated. The subjects' autonomic responses in the 15-second interval between hearing the stimulus word and receiving the shock revealed the existence of unconscious, unverbalizable, anticipation of shock (conditioned anxiety). Cow-shock subjects came to exhibit progressively greater response to the word "cow" than to the word "paper"; paper-shock subjects reversed this trend. This "unconscious anxiety" was not limited to the conditioned stimulus itself but generalized to other words meaningfully related to the conditioned word. Subjects shocked after the word "cow" developed

relative overreaction to other rural words, as opposed to non-rural words.

Of considerable interest and novelty are the facts that the generalized response grew more rapidly and reached a higher level than the conditioned response, and that there was no relationship between extent of conditioning and extent of generalization.

The conditioning curves of aware and unaware subjects differed sharply. Aware subjects immediately developed a strong emergency response that did not grow as a function of the number of reinforcements but instead showed gradual adaptation. Unaware subjects showed typical conditioning and extinction curves at a much lower level of autonomic activity and discrimination.

The generalization curves of aware and unaware subjects were also different. Aware subjects, having been forewarned of the word signalling shock, exhibited generalization to related words in advance of reinforcement. The effect of repeated reinforcements thereafter was more irregular and smaller than for unaware subjects. The generalized response in unaware subjects eventually became and remained greater than in aware subjects.

## TYPES D'ACTIVITÉ NERVEUSE SUPÉRIEURE

PAR

B. M. TÉPLOV (U.S.S.R.)

Les particularités de la psychologie individuelle de l'homme sont directement déterminées par les systèmes stables de réflexes conditionnels, acquis au cours de toute la vie de l'individu.

La formation et le fonctionnement des systèmes de réflexes conditionnels dépendent à leur tour, à un degré considérable, des particularités des processus nerveux (excitation et inhibition), qui se produisent dans l'écorce des grands hémisphères cérébraux. Les particularités essentielles des processus nerveux dans l'écorce sont :

1. l'intensité des processus d'excitation et d'inhibition ;
2. le degré de leur équilibre ;
3. la mobilité de ces processus.

Notre grand physiologiste Pavlov a proposé d'appeler le complexe de ces particularités caractérisant le système nerveux d'un individu donné son type d'activité nerveuse supérieure.

Le tempérament d'un individu est la manifestation de son type d'activité nerveuse supérieure dans son comportement.

C'est sur les particularités du fonctionnement de l'écorce des grands hémisphères que se base la théorie des tempéraments qui est développée dans cet article.

Le tempérament n'est en rapport qu'avec la caractéristique dynamique de l'activité psychique de l'homme.

Toutefois aucun trait de cette caractéristique n'est déterminé exclusivement par des particularités typologiques (intensité, équilibre et mobilité des processus nerveux).

Ces dernières se manifestent toujours indirectement par l'intermédiaire des systèmes de réflexes conditionnels acquis durant toute la vie.

C'est pourquoi le problème des indicateurs des propriétés typologiques essentielles est d'une importance capitale. L'étude de ce problème par rapport à l'homme est une tâche d'actualité qui présente un grand intérêt théorique et pratique pour l'enseignement et la médecine tout d'abord.

On peut diviser les méthodes expérimentales élaborées en vue de déterminer et d'étudier les propriétés typologiques de l'activité nerveuse supérieure de l'homme en méthodes ayant trait aux mouvements volontaires et méthodes traitant des réactions involontaires (réflexes conditionnels vasculaires, réflexe conditionnel de la pupille et autres).

Nous estimons que les méthodes de ce dernier groupe sont beaucoup plus importantes ; en effet, elles seules nous donnent la possibilité d'éliminer l'effet dissimulant des liaisons déjà formées ce qui est le plus essentiel pour dégager expérimentalement des propriétés typologiques.

Parmi toutes les autres méthodes ayant trait aux réactions involontaires la méthode de la réduction de la sensibilité de l'œil à la lumière au moyen des réflexes conditionnels est celle que nous utilisons le plus dans nos laboratoires.

Si on éclaire durant quelques secondes un œil adapté à l'obscurité en faisant fonctionner simultanément un excitant sonore quelconque, après maintes répétitions on obtient la réduction de la sensibilité de l'œil à la lumière par l'excitant sonore seul.

Cette méthode permet d'acquérir des réflexes conditionnels positifs tout comme des réflexes conditionnels négatifs ou inhibiteurs.

Les recherches de nos collaborateurs N. Maisel et L. Schwartz, ont démontré que les sujets soumis à l'expérience diffèrent entre eux quant à la rapidité de l'acquisition des réflexes conditionnels positifs et négatifs.

Une rapidité à peu près égale d'acquisition des réflexes positifs et négatifs témoigne de l'équilibre des processus d'excitation et d'inhibition ; l'acquisition plus rapide des réflexes conditionnels positifs témoigne de la prédominance de l'excitation sur l'inhibition.



Les recherches de I. Ravitch-Scherbo ont démontré que les sujets soumis à l'expérience se distinguent d'une manière non moins frappante en ce qui concerne la rapidité et la facilité avec lesquelles se réalise la transformation du réflexe conditionnel positif en réflexe négatif et vice versa.

Ceci nous indique la mobilité des processus d'excitation et d'inhibition, c'est-à-dire la rapidité et la facilité avec lesquelles ils se succèdent.

Les expériences de V. Rojdestvenskaia établissent les procédés permettant de définir l'intensité du processus d'excitation — cette propriété typologique qui se prête le plus difficilement à l'étude.

L'expérience nous prouve (recherches de N. Leites et autres) qu'on peut développer toutes les facultés socialement indispensables de chaque personnalité quel que soit son type d'activité nerveuse supérieure. Néanmoins, les procédés concrets de développement de ces facultés dépendent des particularités de chaque type. Ainsi on doit tenir compte des traits caractéristiques de chaque type en appliquant une méthode individualisée à l'éducation, à l'enseignement, à la formation du caractère et au développement de toutes les facultés intellectuelles et physiques de l'individu.

## CONDITIONING AND PERSONALITY

BY

HANS EYSENCK (*London*)

There has been a considerable degree of interest in recent years in the relationship between anxiety and conditioning, largely due to the work of Spence and his colleagues. Basing themselves on Hull's formula  $sE_R = sH_R \times D$ , and regarding anxiety as a drive variable, they argue that anxious people should condition more easily than non-anxious people. There is now ample experimental evidence to show that anxiety is related to greater ease of conditioning.

This finding gives rise to difficulties of interpretation. In the first place, work summarized in "*Dimensions of Personality*" and "*The Scientific Study of Personality*" has shown that anxiety has projections on both of the two main dimensions of personality, namely neuroticism and introversion-extraversion; people suffering from overt anxiety belong to the dysthymic group, which is situated in the introverted neurotic quadrant. The relationship between anxiety and conditioning may, therefore, be due to the neurotic or the introverted component, or both; Spence's results do not throw light on this fundamental problem. Secondly, the Taylor



Anxiety Scale used by Spence and his colleagues to measure anxiety has been found to be a rather poor measure of this variable. Hysterics who, as a group, fall into the extraverted neurotic quadrant, score almost as highly on this questionnaire as do dysthymics, in spite of their lack of overt anxiety. Consequently it would appear that this questionnaire is largely a measure of neuroticism, with slight projections on the introversion factor as well. On the basis of available evidence, then, it would appear that Spence's theory essentially links conditioning with neuroticism.

An alternative theory is that of Pavlov, who considers hysteria as being due to exaggerated inhibitory processes, and dysthymia as due to exaggerated excitatory processes. According to him, then, conditioning would be linked with introversion-extraversion rather than with neuroticism.

The crucial experiment to decide between these two theories was performed in our laboratory by C. Franks, who tested 20 normals, 20 dysthymics, and 20 hysterics. On Spence's hypothesis, the hysterics, having high scores on the anxiety scale, should condition almost as easily as the dysthymics, with the normals conditioning least easily. On the Pavlovian theory, hysterics should condition least well, with normals intermediate, and dysthymics conditioning best. Eye-wink conditioning was employed in this experiment as well as P.G.R. conditioning, and both methods gave congruent results in supporting the Pavlovian theory. Combining scores from these two measures, a complete differentiation between dysthymics and hysterics was obtained, with normals intermediate between the other groups.

The Pavlovian theory linking extraversion and cortical inhibition was then generalized to include perceptual phenomena. The satiation phenomena found in Kohler's experiments on figural after-effects may be interpreted as an alternative manifestation of cortical inhibition and, like those of conditioning, may be derived theoretically from Hull's law of inhibition. The prediction was accordingly made that hysterics would show stronger figural after-effects than dysthymics, that they would show them earlier, and that these effects would persist longer for them. These predictions were verified at an acceptable level of significance.

## SYMPOSIUM

### SOCIAL VARIABLES IN PERSONALITY DETERMINATION

#### DIMENSIONS SOCIALES DE LA PERSONNALITÉ

*Chairman — Président*

R. NEVITT SANFORD (*Vassar*)

*Discussion*

ASE GRUDA SKARD (*Oslo*), ROBERT R. SEARS (*Stanford*)

*Papers — Communications*

### PERSONALITY CHANGE IN A SOCIALLY DISTURBED RURAL COMMUNITY

BY

JOACHIM ISRAEL (*Stockholm*)

An attempt is made to define, at least partly, personality in terms of social roles. Social roles are defined as systems of social norms directed toward an individual as a member of a group or representative of a psychologically distinguishable category. A distinction is made between "prescribed role", the former being a system of sent norms and the second a system of received norms. The internalization of subjective roles leads to the development of values. Behavior is then a function of subjective and/or internalized roles. The distinction between "role" and "behavior" is discussed and some of the implications shortly indicated. For example, the results of projective tests are discussed in terms of this distinction.

The conceptual scheme outlined is applied to the personality-structure of the inhabitants of a small rural community in Northern Sweden (Forestville). Through the process of industrialization, which affected this isolated community very late, the social system was disturbed. It is assumed that rigid and limited roles and values render adjustment to the new social situation difficult and create conflicts in the individuals, which in turn increases the existing disturbance of the social system.

SOCIO-ECONOMIC BACKGROUND AS A VARIABLE IN THE  
FORMATION OF ATTITUDES AND VALUES

BY

H. T. HIMMELWEIT (*London*)

The effect of differences in subcultures—in this instance social class—upon the formation of value systems and outlook was examined among three groups of subjects in England: young adolescents, the parents of these adolescents and teachers.

The main inquiry concerned with adolescents was used to examine the validity of Davis' hypotheses about the differential child training patterns in middle and working classes with special reference to his concept of adaptive socialized anxiety. In addition, the study centered around the problem of the transmission of values by examining the relative role played by home and school respectively.

Special attention was focussed upon a hitherto relatively neglected area in this field namely the nature of the interpersonal relationship between parents and children in the different social classes. These were perceived differently by the adolescents from middle and working classes.

While certain of Davis' hypotheses were confirmed, no evidence of greater anxiety amongst middle class children was found. An attempt is made to account for this by reference to the differences in parent child relationship patterns.

Examination of parental values relative to those held by the socially mobile adolescent groups indicates the great complexity of the process of transmission of values and of the relative role played in this process by home and school.

Comparative data obtained from American young adolescents are presented which suggest that there are certain basic consequences of social striving which cut across differences in national values.

SOCIOMETRIC STUDIES ON THE ADJUSTMENT PROCESSES OF  
DISPLACED PERSONS

BY

ELFRIEDE HÖHN (*Tubingen*)

The problem to be discussed concerns the relations between social role and personality. Does the central or peripheral position in a group change personality and in which way does this happen, or is it the personality which determines the social position in the group?

Millions of people from the eastern parts of Germany moved into western areas with the end of and after World War II. Their customs, habits, dialects and social-economic status differed more or less extensively from that of the local population and the already established groups, the expellees had to integrate with. This situation contributed to the accumulation of considerable material for socio-psychological research.

We studied the adjustment process of refugee-children who moved into the same school classes as the natives of the Western parts of Germany. For this study we employed both:

- a) sociograms (repeatedly taken at several months of interval) in order to register changes within the social group, and
- b) case studies mirroring personality changes of the refugee-children.

### RESULTS

During the first phase of the adjustment process individual peculiarities of the personality are hidden behind the peculiarity of the social position. The newcomer is a border figure of the group. The group neither pays attention to him nor rejects him. This causes certain personality changes in the refugee child; he becomes insecure and uneasy, wishing to join the group by any means. That is why he completely adopts the collective norms of the already existing group: like the other children, the newcomer prefers the "stars" and rejects the "black sheep" of the group. This first phase is followed by a critical stage in which the personality of the refugee-child is of decisive nature. The active and well-adjusted children without neurotic tendencies overcome their inferiority feelings of the beginning. They gain self-confidence and strive for contact with the favourites of the class mates. They are helpful and cooperative, thus finally finding a friend and becoming a member of one of the "in-groups" concentrating around the "stars". This successful integrating process influences the behavior and personality of the newcomer. His self-confidence rises considerably, he becomes less tense and more natural, the achievements are getting better and educational problems diminish.

Another category of children, being by nature more passive and reserved have contact difficulties and restricted adjustment capacity. They do not succeed in winning a sufficient contact with the favourites. Finally, they resign and take the remaining possibility of contact: they become friends of the out-siders of the group within the class. From that point on, they themselves become "black sheep" and are rejected by the other group.

According to our observations this development seems to be definite. Teachers are not capable by educational means to change this. Being an

"outside"-member over a longer period changes the personality of the refugee-child characteristically. He becomes withdrawn, defiant, stubborn and loses interest in his school work. Frequently the child becomes outspoken, malicious and difficult. Others become completely timid and unconcerned and finally they may develop all the symptoms of an anxiety neurosis.

#### CONCLUSIONS

The social status in a group and personality are mutually involved in manifold ways. There are certain social situations where the weight is lying on the side of the position in the group, in which the group can change personality decisively. But, how the problem of such adaptational processes are finally solved, will be decided ultimately by the personality of the individual.

### SOCIALISATION ET IDENTIFICATION

PAR

DANIEL LAGACHE (*Paris*)

A la suite des travaux psychanalytiques, on recourt de plus en plus souvent à l'hypothèse de l'identification pour expliquer la socialisation de la personnalité. Les difficultés du problème résident notamment dans le fait que la tendance à l'objectivation s'oppose à l'identification, processus essentiellement subjectif et « anthropomorphisant ».

Une partie de ces difficultés pourrait être levée en recourant au concept de l'interdépendance des rôles. Le terme identification s'applique aux formes les plus opposées de la relation de sujet à objet. *L'acte de s'identifier*, que l'identification soit auto- ou alloplastique, est une relation d'existence qui implique un certain degré de confusion du sujet et de l'objet. *L'acte d'identifier* est un acte de connaissance qui implique la distinction du sujet et de l'objet (objectivation).

Dans l'histoire de la psychologie comme dans le développement individuel, l'objectivation (ou identification objective) se constitue secondairement, par exclusion progressive de toute identification subjective et interpersonnelle ; d'où notre difficulté d'explorer celle-ci. Les formes les plus primitives de la conscience peuvent être considérées comme des formes d'identification auto- et alloplastique ; il ne peut en être autrement si l'on admet à la fois que la conscience est conscience de quelque chose (intentionnalité) et que la structure primitive de la conscience est « adualistique ». Les travaux psychanalytiques, en particulier ceux de l'Ecole de Mélanie Klein, font jouer un grand rôle aux mécanismes d'introjction

primaire et de projection primaire, à un stade de pré-objet antérieur à la verbalisation. Plus faciles à saisir sont les identifications contemporaines d'une expérience plus organisée et de l'acquisition du langage. L'observation des relations interpersonnelles des enfants de 1 à 3 ans et de leurs manifestations linguistiques montrent que, ce qui est intériorisé, ce n'est pas seulement l'image d'un Socius, mais une relation interpersonnelle, par exemple une relation à deux comportant des rôles complémentaires. Cette identification contrastante donne sa structure dualiste à l'expérience intérieure et explique que, dans les relations interpersonnelles ultérieures, le sujet puisse, selon la situation, jouer tantôt un rôle, tantôt le rôle complémentaire (exemple : mère-enfant, autorité-soumission).

Enfin, l'identification du sujet à son propre Ego peut être conçue aujourd'hui comme l'intégration d'identifications multiples, par la prédominance de l'identification à un objet privilégié. Les identifications secondaires sont les modifications structurantes et durables de la personnalité qui résultent des identifications primitives. La conceptualisation la plus connue est la conceptualisation freudienne (Id, Super-Ego, Ego-Idéal, Ego). Elle est probablement au-dessous de la complexité des faits. On s'efforce de préciser la distinction entre Super-Ego et Ego-Idéal, en la rattachant à l'hypothèse de la structure interpersonnelle des identifications : l'Ego-Idéal connote la façon dont le sujet doit se comporter et se percevoir pour se conformer aux motivations connotées par le Super-Ego.

L'identification est un des aspects les plus importants du transfert ; toujours en utilisant la complémentarité des rôles et le caractère dualiste des identifications, on montre que l'identification dans le transfert ne s'applique pas seulement au Socius (ou au psychanalyste), mais que le sujet se projette également dans un personnage et un rôle (identification aliénante).

Le développement mental comporte la réduction des identifications au profit de l'objectivation. Passé un certain niveau, l'identification est-elle encore possible, en l'absence de régression ? joue-t-elle un rôle utile ? Une réponse positive est donnée, sur la base d'une analyse de la communication verbale et de ses difficultés. On conclut que les processus d'identification pourraient fournir la base de dimensions mesurables de la personnalité : 1) identification contre objectivation ; 2) identification autoplastique contre identification alloplastique ; 3) identification aliénante de l'Ego, etc.

En conclusion, notre tendance à l'objectivation tend à nous faire méconnaître le rôle de l'identification, comme processus par lequel l'homme devient semblable à l'homme, et auquel il ne peut complètement se soustraire.

## SYMPOSIUM

### MECHANISMS OF MOTIVATED BEHAVIOR

### MÉCANISMES DE MOTIVATION DANS LA CONDUITE

*Chairman — Président*

CLIFFORD T. MORGAN (*John Hopkins*)

*Discussion*

DANIEL E. BERLYNE (*Aberdeen*), JUDSON S. BROWN (*Iowa*)

*Papers — Communications*

### HYPOTHALAMIC MECHANISMS IN MOTIVATED BEHAVIOR

BY

ELIOT STELLAR (*Pennsylvania*)

The present research program is based on the notion that motivation is under multifactor physiological control—sensory, chemical and central nervous. Of particular importance in this multifactor mechanism is the hypothalamus, for it appears to be the region of the brain which integrates the contributions of these diverse factors and controls the final common paths to motivated behavior.

Most of our research up to now has been on changes in the sensory control of hunger following hypothalamic lesions. But work is under way on certain specific hungers, thirst, and sleep and a start has been made on the contribution of the internal environment to the hypothalamic control of motivation.

The first experiments on hunger deal with the effects of ventromedial lesions. Animals with this lesion overeat and become obese on standard laboratory diet. But if their food is diluted with non-nutritive cellulose, they fail to maintain caloric intake like normal rats and may even refuse to eat. This effect is partly due to the powdery texture of diluted diets as a comparison of the intake of food pellets and powder, ground of the pellets, shows: hyperphagic rats take less of the powdery diet whereas normal rats do not.

These findings suggest that ventromedial lesions release the eating responses from inhibition in two directions, yielding exaggerated positive reactions (overeating) to certain 'positive' aspects of the diet and exaggerated negative reactions (refusal to eat) to certain 'negative' aspects of the diet. To check this notion, the rats were tested with diets adulterated with quinine (negative stimulus) and dextrose (positive stimulus). At the concentrations of adulteration used, the intake of normal rats was not affected. But the hyperphagic rats rejected the quinine diet almost completely and increased their intake of the dextrose diet greatly.

These results apply to obese rats only. If obesity is prevented by restricted feeding, the failure to regulate caloric intake still shows up, but there is no "finickiness".

The second group of experiments on hunger deals with lesions of the lateral hypothalamus. After this operation, rats fail to eat completely and starve to death. But they can be kept alive by stomach-tube, and after some time, will recover eating behavior. The course of recovery is most instructive. For about a week after operation, these rats eat no food at all. About this time, they accept only chocolate or evaporated milk. Later they begin drinking water, and after a period of several weeks to several months, accept the standard laboratory diet. Here, too, the hypothalamic lesion has changed reactivity to sensory stimuli provided by food.

A different approach to the problem of hunger, and also of thirst, is to manipulate the hypothalamus chemically through chronically-implanted pipettes. This method not only provides a way of stimulating and depressing the hypothalamus but it also offers the chance to determine whether certain chemicals like insulin and amphetamine, that are important in hunger, have their site of action at the hypothalamus. The method and preliminary results are reported.

## EXPERIMENTAL ANALYSIS OF EMOTIONAL BEHAVIOR

BY

JOSEPH V. BRADY (*Washington*)

A series of experiments concerned with the effects of electro-convulsive shock (ECS) upon acquired emotional sensitivities and responses in animal subjects provided the initial impetus for the research program of which this paper constitutes the most recent report. Earlier experiments with rats have shown that the strength of a conditioned emotional response (CER)



of the "fear" or "anxiety" type, consisting of a cessation of lever pressing in a Skinner Box upon presentation of a clicking noise, can be markedly attenuated by a series of ECS treatments. This effect is temporary, however, for the CER can be demonstrated to reappear within 30 days following ECS. Furthermore, when the ECS is delayed for as long as 30 days after conditioning of the CER, it is much less effective in attenuating the response. More recently, additional experiments point to the convulsion *per se*, as differentiated from the passage of electricity, as a necessary and sufficient condition for attenuation of the CER, and indicate quite clearly that the effect of ECS upon the CER is definitely a function of the temporal distribution of the ECS treatments.

In a more direct attempt to investigate the neural correlates of such emotional reactions, the effects of both cortical and subcortical ablations upon the CER and a simple avoidance response in rats and cats have been studied. Lesions in the septal forebrain area of the rat, in addition to producing marked increases in emotional reactivity, at least partially attenuate the strength of the CER although no such efforts are demonstrable following cingulate lesions. Lesions involving the habenula nuclei of the thalamus in the rat, on the other hand, while producing neither increases in emotional reactivity nor decrements in acquisition or retention of the CER, do appear to hasten extinction of such emotional responses. Further experiments in cats utilizing an avoidance procedure indicate that lesions of the amygdala complex before training produce severe decrements in acquisition of a conditioned avoidance response although such lesions do not appear to affect retention of an avoidance response acquired preoperatively. Lesions of the orbitofrontal cortex in cats, however, do result in complete loss of a previously conditioned avoidance response with reconditioning possible only in some animals after prolonged training.

Most recently, studies have been initiated to investigate the functional relationships between various aspects of the organism's behavioral repertoire and emotional conditioning procedures involving "punishment", "fear", "avoidance", and "conflict". Results obtained thus far indicate quite clearly that both the rate of acquisition and resistance to extinction of the conditioned "fear" response are a function of the intensity of the unconditioned stimulus and the number of conditioning trials. Furthermore, the rate of extinction of such a "fear" response has been found to be a function of the positive reinforcement schedules maintaining the competing lever pressing behavior upon which the CER has been superimposed. Finally, recent attempts to study the differential effects of various pharmacologic agents including benzedrine, morphine, alcohol,

and sodium pentobarbital on such conditioned emotional responses promise to provide another useful tool for the experimental analysis of such complex behavioral phenomena.

## TRAUMATIC AVOIDANCE LEARNING

BY

RICHARD L. SOLOMON (*Harvard*)

Recent experiments on traumatic avoidance learning have produced results which pose serious problems for existing theories of motivation and learning. The results which seem most crucial come from: (1) analyses of the relationships between the development of emotional reactions and instrumental responses in the acquisition phase of traumatic avoidance learning; (2) the analysis of the relative degree of success in obtaining extinction of learned avoidance responses by using different extinction procedures; and (3) the experimental analysis of temporal relationships between events during acquisition and extinction of avoidance responses.

In the analysis of the acquisition of avoidance responses, using intensely noxious stimulation for motivation, it is clear that conditioned anxiety reactions can be established relatively independently of reinforced instrumental acts. Here, the CS-US interval is a controlling variable, as is US intensity. It is also clear that learned avoidance responses can be established independently of the establishment of a definite CS-US relationship. An important controlling variable is the time interval between the instrumental act and the removal of pain-fear. Pain onset seems most relevant for the establishment of the emotional reactions, pain termination for the maintenance of the instrumental response. There seem to be two distinct processes involved.

The analysis of the outcomes of extinction procedures also points to two processes, but here they interact to prevent the rapid attainment of extinction. The conditions needed for the extinction of conditioned anxiety reactions are different from the conditions needed for extinction of the learned avoidance responses. The operation of "reality testing" procedures can result in strengthening avoidance responses rather than weakening them, through the operation of the principle of "anxiety conservation".

Experiments on variations of the CS-US interval, variations of CS duration, and variations in delay of shock termination point to two major processes underlying the maintenance of avoidance responses. One process is emotional, the other adaptive with regard to the emotional state and its elicitors.

## DRIVE, DRIVE-REDUCTION, AND REWARD

BY

NEAL E. MILLER (*Yale*)

A combination of physiological and behavioral techniques opens up new possibilities for studying motivation and reward.

One series of experiments is investigating motivational effects of electrical stimulation of various points in the brain. This work is in collaboration with Dr. Jose M. R. Delgado, Yale Department of Physiology, and Mr. Warren Roberts, Yale Department of Psychology. A chronic operation implants in the brains of cats electrodes connected with miniature sockets projecting from the head. The experimenter can stimulate a normal, ambulatory cat and elicit emotional responses which can be conditioned and can be used to motivate the learning and performance of new habits.

One experiment showed that the emotional disturbance elicited by stimulating a point in the brain could be used to motivate a cat to learn to turn a wheel, and that escape from this disturbance served as a reward to reinforce turning the wheel. Then the central stimulation was used to condition the wheel turning to new cues such as a tone or flickering light.

Another experiment used central stimulation to condition an emotional disturbance to one of two distinctive boxes. Then, on trials without further stimulation, the animals learned to escape from the "frightening" into the "safe" box. Control animals stimulated in the motor cortex with higher voltages (producing more violent motor responses) were not emotionally disturbed in the box and did not learn to escape.

After control stimulation of the motor cortex produced a violent withdrawal from food, hungry cats resumed eating; a weaker stimulation at the emotional point acted as a punishment teaching hungry cats prolonged avoidance of food.

These experiments, which were shown in the film session, demonstrate that the central stimulation functions as a drive; its termination serves as a reward.

Another series of experiments investigated hunger. In chronic operations little plastic tubes were sewn into the stomachs of rats, threaded under the skin, and allowed to project from the back of the neck. Mrs. Marion L. Kessen, Dr. Martin Kohn, and Mr. M. M. Berkun of the Yale Department of Psychology collaborated in these experiments. A control solution of normal saline injected directly into the stomach had relatively little effect on rate of working for food or amount consumed.

Food injected directly into the stomach promptly reduced hunger, and food taken normally by mouth produced an even greater reduction. Distension of the stomach by a balloon reduced the rate of working for food; injection of an equal volume of milk into the stomach produced a greater reduction. Experiments using a T-maze showed that food injected directly into the stomach serves as a reward to produce learning, food taken normally by mouth is a stronger reward, but distension of the stomach by a balloon acts like a punishment.

We conclude that a number of mechanisms regulate hunger: it is reduced by food in the mouth or food in the stomach; stomach distension produces motivation (perhaps nausea) conflicting with hunger.

### EXPLORATORY DRIVES IN PRIMATES

BY

HARRY F. HARLOW (*Wisconsin*)

Psychologists in the past have placed primary emphasis upon internal drives, such as hunger and thirst, and upon nociceptive drives (specifically pain) as the most fundamental behavioral motivating agencies. During the last five years a series of researches designed to investigate the role of a group of other motivational agencies, which may be classified as exploratory drives, has been conducted at the Wisconsin laboratory. This class of drives has been demonstrated to include both manipulation and visual exploration, and it probably also includes auditory and somatic exploration. It is postulated that these drives may be as basic, persisting, and important as the internal drives. In all probability the exploratory drives increase in range and intensity as one ascends the primate order. The Wisconsin data have recently been supplemented by research along similar lines from other laboratories.

The Wisconsin studies have demonstrated that monkeys manipulate puzzle devices and that this behavior is persisting even though the animals receive no reward other than that afforded by the act of manipulation. Furthermore, repeated exposure to either three—or six device puzzles results in increased puzzle proficiency which may culminate in nearly errorless performance. Similar learning based on manipulation motivation was demonstrated in one available chimpanzee. Manipulation was also utilized as a source of motivation for solution of a series of discrimination problems by four rhesus and one cebus monkey.

A technique for measuring the visual exploration drive in monkeys was

developed by Dr. Robert Butler, and researches were conducted on the strength and persistence of this motive and on learning utilizing this motivational source. Learning to visual exploratory drives was repetitively demonstrated, and the persistence of the drives was indicated by continued responsiveness for many hours.

These data suggest that there exists a family of drives which have heretofore been relatively neglected by psychologists; a family of drives which may be fundamental to understanding human and sub-human motivation and resulting learning.

## SYMPOSIUM

### SOME NEW APPROACHES TO THE MULTIDIMENSIONAL ANALYSIS OF BEHAVIOR

### QUELQUES NOUVELLES MÉTHODES D'ANALYSE MULTI- DIMENSIONNELLE DU COMPORTEMENT

*Chairman — Président*

CARL I. HOVLAND (*Yale*)

*Discussion*

HANS J. EYSENCK (*London*), HOWARD RAIFFA (*Columbia*),  
GÖSTA EKMAN (*Stockholm*)

*Papers — Communications*

### THE CONJUNCTIVE, DISJUNCTIVE, AND COMPENSATORY MODELS FOR COMPLEX BEHAVIOR

BY

CLYDE H. COOMBS (*Michigan*)

Psychological theorizing may be regarded as concerned with how complex behavior is constructed out of components, these components usually being attributes of stimulus situations and attributes of the individual. Experimental research is usually directed at discovering such components and determining something about their relative importance.

The model which is almost universally used for this purpose may be called the compensatory model in which the complex attribute is constituted from a weighted combination of the components; usually linear but other functions are sometimes considered. In essence, an individual may successfully perform on a task by compensating for a shortage of one component with an excess of another. This is the model of multiple regression, multiple factor analysis, and analysis of variance.

In contrast to this model, there are some new ones called the conjunctive and the disjunctive models. In the conjunctive model successful per-

formance requires a certain minimum of *each* of the components; a deficiency in any one leads to failure. In the disjunctive model successful performance requires a certain minimum of *any one* of the components; a deficiency in all leads to failure.

These models have been formally constructed with the necessary theorems for determining dimensionality and configuration. They lead to nonmetric forms of factor analysis in which one may go directly from the response patterns to factors on which the stimuli and individuals are simply ordered without the assumptions involved in numerical scores and correlation coefficients.

## A GENERALIZED SIMPLEX FOR FACTOR ANALYSIS

BY

LOUIS GUTTMAN (*Israel*)

In a new approach to factor analysis, called *radex* theory, it has been shown previously how two important special cases arise: the *simplex* and the *circumplex*. We shall generalize here the concept of the simplex beyond the restricted case presented previously. This will indicate possible generalizations also for the circumplex and for a complete radex.

In the restricted case studied previously, the scores of individuals on the underlying simplex were expressible as functions of underlying factors in several alternative ways. The *additive* way assumes a set of 'n' uncorrelated factors. This has now been found empirically to account rather well for correlation matrices published earlier in the literature by many research workers on many types of mental tests, as well as on some personality questionnaires and other types of data. Conventional multiple-common-factor techniques used by these other workers have failed to reveal this simple orderliness where it exists. For example, it has now been shown that the correlations among the following set of six tests of Thurstone can be accounted for approximately by an underlying simplex: addition, subtraction, multiplication, division, arithmetical reasoning, arithmetical judgment. This implies that performance in subtraction involves what addition does, but also something more. Multiplication involves both, but also something more; etc. Some further generalizations and their consequences are explored.

THURSDAY, JUNE 10 — JEUDI LE 10 JUIN

## SYMPOSIUM

### CONSCIOUSNESS: REVISED AND REVIVED

#### LA CONSCIENCE REVALORISÉE

*Chairman — Président*

ROBERT B. MACLEOD (*Cornell*)

*Discussion*

Sir FREDERIC C. BARTLETT (*Cambridge*), EDWIN G. BORING (*Harvard*),  
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*Papers — Communications*

### CONSCIOUSNESS AND THE PHENOMENOLOGICAL APPROACH TO PSYCHOLOGY

BY

ALBERT WELLEK (*Mainz*)

The phenomenological method in psychology is descriptive and consequently aims at "Verstehen" (understanding of meaningful structures). It has various forms and different fields of application. There is:

1. phenomenology "from inside" or from the internal viewpoint, i.e., the description of direct experience and introspective phenomena;
2. phenomenology "from outside" or from the external viewpoint, derived from objects
  - a) as objects of expression, which leads back to introspective analysis (see 1. above),
  - b) as objects of appearance or apparent objects, i.e. of phenomena as such.

Conforming to the possibility mentioned under (b), personality traits and the principles of the structure of characters the developmental aspects



of psychoanalysis, etc. can be the subject matter for phenomenological analysis as well as colors, feeling, thinking, willing, imagination, the physiognomic qualities, or any other processes and states of consciousness. Without the application of the phenomenological method, all these facts can neither be sighted nor understood with regard to their specific qualities and meaningful connections. This means that a psychology without phenomenology is no psychology at all, nor is it any longer a satisfying science which can be developed to some sort of perfection. For example, it would be impossible to establish either characterology, psychoanalysis, or characterological diagnosis. These necessarily depend upon the phenomenological method, even where the technique has pretensions of exactitude in the usual sense of the word (see Rorschach, TAT).

As a rule, we can find experimental verification *ex post* for the phenomenological demonstration. This is true also of characterology, even though the problem of experimental approach presents great difficulties in this field. For instance, the theoretical concept of the existence and the structure of a "character core" which various authors have reached through purely phenomenological means, can be experimentally verified in hypnosis by the occurrence of a "character barrier". The invasion of the postulated "core" is regularly resisted either by a severance of the *rapport* or by a refusal to fulfill the post-hypnotic command. This is done every time in a different manner according to the peculiarity of the hypnotized person. Thus, experimental hypnosis becomes a means for character diagnosis. The behavioral findings (in hypnosis) will verify what had been found before in phenomenological demonstration.

It is a methodological circle to say that one can consider character traits and personality structures as existent only if we have "measured" them by specific tests. The test does not tell me what it measures, but I tell the test what it measures.

It is true that the phenomenological method is not exact in the same sense as the inorganic sciences; nevertheless it is objective inasmuch as it leads to intersubjective and essential results, when it is practised correctly. Both the "operationalist" and the phenomenologist are right, each according to his own method. Their concepts of knowledge cannot be entirely different, as has been recently argued. Not only can and should the phenomenologist satisfy the operationalist by providing him with experimental proof and statistical data, but the operationalist also must submit his results (e.g. of a "factor analysis") to phenomenological control and interpretation. No system of psychology can be possible unless these two methods or aspects can be brought into agreement.

## CONSCIOUSNESS, BEHAVIOR AND PERSONALITY

BY

JOSEPH NUTTIN (*Louvain*)

Psychologists who work in the fields of cognition and perception still frequently consider the perceived world as an internal "representation" of a real external world. This "representation" theory is strongly rooted in psychology, but it originates in a philosophical speculation which raises the question of a "reality" underlying the objects of cognition. From the viewpoint of the psychological study of behavior, the perceived world is by no means a representation *inside* of man (or inside consciousness) of some object whose real existence lies outside of him. The perceived world is the real world itself and perception consists of direct contact with the environment. For this reason the notion of consciousness, as it has been developed within the framework of introspective psychology, is in need of revision. Consciousness does not refer primarily to an internal world made up of representations; it consists of a direct awareness and of an immediate presence of the world itself.

Consciousness therefore signifies before all else "exposure to the real world" and not "confinement in an internal world of representation".

Consciousness establishes man as a being who lives and behaves in the world, thus consciousness is an essential aspect of behavior.

As to the concept of personality, nothing is less in conformity with reality than to conceive of personality as a pure internal structure of traits, aptitudes, etc. *Personality* signifies a specific modality of existence and of activity which is characterized by the fact of involvement in a *situation* or in the *world*. The fundamental structure is therefore a unity, an "ego-world", and not an internal organization of an ego. If, as Lewin says, it is true that the personality governing behavior is a part of the psychological field, it must not be forgotten that the psychological field itself, more basically, enters into the make-up of personality. Motivation also must be looked at in relation to the fact that man lives in a meaningful world. To realize himself in this world, and to be integrated therein appears to be the fundamental aim of his motivation.

The science of behavior and of personality therefore can not make an abstraction out of the concept of consciousness, in the sense of exposure to the world. The introduction of the notion of consciousness solves no concrete scientific problem regarding behavior, but it is necessary to appeal to the concept in order to formulate and to come to grips with the problems of behavior and of personality in a realistic fashion.

CONSCIOUSNESS, THE PERCEPTUAL WORLD, AND  
INTERACTION WITH OTHERS

BY

FRITZ HEIDER (*Kansas*)

The problem of consciousness gains a special significance if we consider the relations and interactions between people. If two persons interact, one person can perceive the other one perceiving or intending to do something, his emotions may refer to and interact with the other person's emotions. They both are open to the other's openness to themselves.

We do not react in the same way to persons and things; persons and things do not play the same role in our perceptual world. This difference is not produced simply by the fact that other persons have consciousness. It can only be understood if we relate it to the fact that entities endowed with consciousness do not function in the same way as things function. Persons and things play a different role in the causal structure of the environment. For instance, persons show equi-finality or distal invariance in their actions, a person can attain his ends in spite of variation in the means. In other words, a person shows cognition and purpose. Cognition and purpose has been considered with the cognizing and behaving organism as reference point. Here we have to consider cognition and purpose with the recipient of purposeful behavior as reference point. What does it mean to a person when his satisfactions and frustrations form the goals of another person? Such considerations show that the way in which we naively experience other persons in our life space represents more or less adequately the specific way of functioning of other persons. The idea of personal causality or of personal responsibility, is justified by the pattern of events which purposive behavior implies. We do not react in the same way when we are threatened by another person or when we are threatened by something that does not act purposefully, and we do not experience the threat in the same way. Similarly, being perceived by another person, or being an object of his cognition has its specific meaning for us because of the functions of perception, that is, the effects perception has on the behavior and attitudes of the other person.

Thus all interactions between people have to be understood in terms of the fact that they are mutually "open" to each other, and at the same time open toward the same environment.

CONSCIOUSNESS AND THE UNCONSCIOUS IN A MODEL OF  
THE HUMAN BEING

BY

SILVAN S. TOMKINS (*Princeton*)

The dubious status of the concept of consciousness is a derivative of a theory of causality—a theory which assumes that the relationship between events is either determinate or capricious. When the ideological barricade has been so raised, no scientist will waver. Strict allegiance must be exacted by any concept which has but two values. Instead of asking the question determinate or indeterminate, of the relationship between any two events, one may ask the question possible or impossible? It then appears that 'a' followed by 'b' may be impossible, or at least never observed, or that 'a' may be followed by 'b' if 'a' has been preceded by 'c', but not otherwise. The two ends of this longish continuum would be that nothing is possible and everything is possible. We will call one pole complete redundancy—nothing can be changed. The other pole we will call pure randomness—everything can be changed in any way. This is our own definition of a random series. Causal relations can thus be ordered to a continuum of complexity. By complexity we mean the number of independently variable states within a system. The more homogeneous the system, the more redundant—the more heterogeneous, the greater the degrees of freedom in the system.

Consciousness, we propose, is a type of duplication in nature. One part of space duplicates itself in another part of space. A living system seems to provide the necessary but not sufficient conditions for the phenomenon. A living system, whether conscious or not, is also capable of duplication. A part of the system is capable of duplicating not only itself, but also the whole of which it is a part in such a way that an infinite progress is possible. When we change the direction of the time arrow we are confronted with paradox. The relationship between the germ cells and the rest of the body is similar to that between the body as a whole and the environment, when there is consciousness. Duplicability is a necessary condition for any increment in transformability or complexity in any system, formal or empirical. In a formal system such as arithmetic there is an implicit assumption of duplicability that there are as many ones for example, as the mathematician may ever need. In other formal systems, such as games, the number of elements of each kind, say pawns in chess, is stated to be some specific number. With every increase in number of duplicates a game becomes more complex. Similarly organic compounds

outnumber all other chemical compounds in the ratio of about ten to one. The existence of this large number of organic compounds is due to the unique capacity of carbon atoms to unite with themselves to form rings of more or less complexity.

The role of consciousness is the enabling of a more informed mobility. In this respect it is similar to the achievement of a constant internal temperature. It is not the only way in which the organism might have been informed. How to clot a bleeding finger is knowledge which we may or may not inherit genetically. It is conceivable that nature might have known herself well enough to build into mobile organisms, knowledge and competence to deal effectively with future contingencies.

Although we believe that consciousness is one of the most important capacities of the human being, it needs to be assigned a place and function within a more general model of the human being. We believe the human being can be best understood as an intercommunication system for the reception, transmission, translation and transformation of messages, conscious and unconscious. Who says what to whom must be specified as well as what may not be said. In such a venture the analysis of language transformations assumes a critical importance. Man owes his superiority not to his possession of language. He owes his possession of language to the superiority of his nervous system.

**SYMPOSIUM**  
**CEREBRAL FUNCTIONS AND BEHAVIOR**  
**COMPORTEMENT ET FONCTIONS CÉRÉBRALES**

*Chairman — Président*

DONALD B. LINDSLEY (*California*)

*Discussion*

WARD C. HALSTEAD (*Chicago*), ROBERT B. MALMO (*McGill*),  
HANS-LUKAS TEUBER (*New York*)

*Papers — Communications*

**CORRELATES BETWEEN PSYCHOLOGICAL PROCESSES AND  
THE ELECTRICITY ACTIVITY OF THE BRAIN**

BY

HERBERT H. JASPER (*McGill*)

Relationships between psychological processes and the electroencephalogram will be discussed with respect to (1) general states of consciousness, (2) emotional states, "tension", or anxiety, (3) the process of attention, (4) specific sensory or perceptual processes, (5) anticipatory set in learning, and (6) motor functions with particular reference to the initiation of "voluntary" movement.

Definite relationships can be shown with all of these psychological processes, but there are many exceptions and contradictory findings not always readily explained. For example, slow waves are generally present with impairment of consciousness, but they may be present without loss of consciousness as well. Rapid waves characterize the state of impaired consciousness in light barbiturate anesthesia. One reason for these discrepancies is the relatively small sampling of the electrical activity of the brain recorded in the E.E.G. This is proven by microelectrode studies of the electrical activity at different layers within the cortex and by studies

of independent activity from deep lying cortical or subcortical structures whose activity is inaccessible to the electroencephalogram.

To these serious technical limitations must be added the inadequacy of definition of psychological processes and our lack of sufficient fundamental knowledge regarding the functional significance of "brain waves" from a neurophysiological point of view. It is now clear that they do not represent the actual discharge of cortical cells. They represent only a small sampling of the synchronous changes in "field potential" in aggregates of ganglionic networks which may serve to facilitate or inhibit or to time neuronal cell discharge. The regulation of this type of cortical activity by the unspecific projection system of diencephalon and brain stem, which, in its turn, receives projections from the cortex, may be of far-reaching consequence for integrative functions of the brain, conscious or unconscious, though possible relationships to conscious psychological processes is of paramount interest at the present stage of research.

Difficulties with definitions of "conscious" processes are immediately apparent. Examples are given of various types of impairment or loss of functions to emphasize the need for precise definitions of the nature of the particular process affected before further progress can be made in the attempt to correlate psychological processes with the electrical activity of the brain.

## EFFECT OF ELECTRICAL STIMULATION OF THE BRAIN ON THE BEHAVIOR OF MONKEYS

BY

H. ENGER ROSVOLD and JOSÉ M. R. DELGADO (*Yale*)

*Effect of Electrical Stimulation of the Frontal Lobes on Delayed-response Type Test Performance:* Twelve rhesus macacus were trained in delayed alternation and discrimination tasks. The trained animal went first to the left, then to the right, to the left and to the right and so forth for 50 trials, making not more than one error. When trained to discriminate he went without error to a cup with black striations on a yellow background rather than to a cup with a black triangle on a yellow background. A four-point electrode was then placed bilaterally at various points in the brain by means of the Horsley Clarke apparatus.

After retraining, the animal was tested while the brain was being stimulated. On different days different points were stimulated in a

balanced order, one point each day until all the points were stimulated several times.

When some points were stimulated the performance on the delayed-response type test was disturbed while discrimination test performance was not. Stimulation of other points, though frequently evoking marked motor effects, did not disturb performance on either test. Points which appear to be critical for the effect were shown upon histological examination to be in the most anterior portion of the head of the caudate.

*Effect on Conditioned Fear of Stimulating Mesencephalic, Diencephalic and Temporal Lobe Structures:* In this experiment, the animal was trained to turn one cup over in order to turn off an electrical current which was painfully shocking him through the feet. He was taught to turn the other cup over to obtain a peanut which had been placed underneath. Training then proceeded to associating the shock with a high tone and the food with a low tone. The final stage of training was accomplished when the animal "anticipates"; i.e., as soon as the high tone comes on, to turn this cup to avoid the shock which begins two seconds later, and when the low tone or no tone at all comes on, to turn the other cup within two seconds and get food. The trained animal invariably turned the *fear* cup when he heard the high tone and invariably the *food* cup when he heard the low tone, or no tone.

Ten rhesus macacus were used in this part of the study. Electrodes were implanted so that small areas in mesencephalon, diencephalon, and in temporal lobe could be stimulated separately. On the stimulation trials stimulation through the electrodes was substituted for the conditioned stimulus; otherwise the trial was identical with a *food* trial. It was possible to evoke the conditioned fear response by electrical stimulation from some points but not from others, indicating that the effect of stimulation was specific to the locus at which the stimulation was being applied. The effect was obtained from specific points in the pons and mesencephalon. In addition it was obtained from stimulation of central grey. Stimulation of other points, though sometimes evoking other responses, did not evoke the conditioned fear response.

These results are discussed under two topics: (a) the similarity between the effects of stimulating the frontal lobes and of frontal lobotomy and (b) the relationship between conditioned fear evoked by electrical stimulation and fear (anxiety) as it is generally considered.



## RIGHT-LEFT IDENTIFICATION, FINGER-LOCALIZATION AND CEREBRAL STATUS

BY

ARTHUR L. BENTON (*State University of Iowa*)

The relationship of right-left identification and finger-localization to cerebral function has been a subject of interest ever since the clinical significance of impairment of these behaviors was first established. Both types of behavioral deficit have been ascribed to focal lesions in the parietal area of the dominant hemisphere.

There are a number of interesting features associated with the two deficits. They provide rather striking examples of relatively circumscribed cerebral lesions which apparently cause appreciable impairment of complex performances, thus presenting some challenging facts for which account must be made in any endeavor to conceptualize the relations between brain and behavior. The finding that the responsible lesions are practically always to be found in the dominant hemisphere raises the question of the exact limits of the significance of lateral cerebral dominance in behavior. Finally, the concurrence of the two deficits, as well as their association with impairment in writing and calculation, poses a theoretical problem for which a number of answers have been offered.

With the rather gross methods employed in the typical neurological examination, isolated impairment in right-left identification and finger-localization is not a frequent finding in adult patients with cerebral disease. However, utilizing examination methods which are somewhat more refined than those typically used, Benton, Hutcheon and Seymour (1951) found a considerable incidence of defective performance in brain-injured and defective children and adolescents. Quantitative analysis of the performances of this limited sample of subjects yielded suggestive evidence of a significant association between the two abilities.

The series of studies here reported have as their aim the elucidation of some of the questions raised by earlier findings. The genetic development or right-left identification and finger-localization in normal children has been systematically investigated. More extensive data on the performances of brain-injured children have been gathered. The performances of a series of patients with cerebral pathology have been compared with a group of control patients. Finally, the interrelations of the two abilities have been analyzed.

The findings have been as follows:

- (a) With respect to both right-left identification and finger-localization,)

there is a systematic growth in ability from ages 6 to 10 in normal children. With respect to right-left identification, some children show a reverse association between verbal symbol and side of body, i.e., these children discriminate well between the two sides of the body but consistently attach the incorrect verbal symbol to each side.

(b) A substantial proportion of brain-injured and defective children, matched for mental age with the normative group, make extremely defective performances on both the right-left identification and finger-localization tasks.

(c) In contrast to the findings with brain-injured and defective children, impairment in these behaviors is a relatively rare finding in adult patients with recently acquired cerebral pathology, a result which confirms general clinical observation.

(d) When the influence of chronological and mental age is controlled, a significant correlation of rather small magnitude is found between the two abilities. To this extent, the assertions of clinical theorists that there is a common factor involved in the two functions are confirmed.

#### PROBLEMS OF LANGUAGE DISTURBANCE IN CEREBRAL "DYSFUNCTION"

BY

PIERRE J. PICHOT (*Paris*)

Technical difficulties in the experimental study of language disturbances are discussed. It is difficult to construct homogeneous tests which discriminate at the same time among normal subjects and among subjects with language disturbances. In addition, the tests must be controlled with respect to a number of variables. Vocabulary, for example, varies with cultural level (even when general intellectual level is kept constant), with age, with sex, and with the type of test used. Other difficulties result from the contradictory results of factorial analysis. An examination of the conceptions of language of the bifactorial and the multi-factorial schools indicates that they are hardly adequate to provide a basis for the study of language disturbances.

Language disturbances in aphasia are then discussed. The roles of visual agnosia, of auditory agnosia and of the phonetic disintegration syndrome are summarized. The central aphasic disturbance is considered in the light of the Baillarger-Jackson principle. It is concluded that linguistic functions and intelligence are relatively independent of each

other and that there may exist a relatively localized cerebral basis for linguistic functions.

Language disturbances in dementia are considered from the point of view of the preservation of vocabulary and of the decrease of scores in non-verbal intelligence tests. The principles underlying mental deterioration tests, particularly the existence of a "vocabulary superior to non-verbal intelligence" pattern in dementia, are discussed.

The implications for some particular clinical problems are examined. In arteriopathic dementia there exists a decline of vocabulary which is greater than that observed in senile dementia. This suggests the hypothesis that in the arteriopathic type of dementia, there also exists a "latent aphasia", superimposed on the basic dementia, the "latent aphasia" being associated with relatively localized lesions. In epileptic dementia, one observes a relative inferiority in level of verbal functions. The role of educational restriction and of "latent aphasia" in patients with temporoparietally focalized epilepsy is discussed.

It is concluded that the type of language disturbance which is characterized by the "vocabulary superior to non-verbal intelligence" pattern is associated clinically with dementia and anatomically with diffuse cerebral damage. The inversion of this pattern is the result of aphasic disturbances evidencing the existence of localized brain damage. The possibility of the addition of such localized lesions to cases of authentic dementia should lead to caution in the interpretation of the results of experimental studies based on non-homogenous groups and in the practical use of so-called mental deterioration scales.

## EFFECTS ON PERSONALITY OF EXCISIONS IN DIFFERENT REGIONS OF THE BRAIN

BY

ASENATH PETRIE (*London*)

Changes in personality after five types of brain operation are compared. These operations were: (1) a complete standard lobotomy; (2) a partial rostral lobotomy; (3) circumscribed excisions in the region of the convexity (Brodman's Areas 9 and 10); (4) circumscribed excisions in the cingulate area (Brodman's Area 24); (5) circumscribed excisions in the orbital region (Brodman's Areas 11 and 47).

The changes in personality have been measured by objective tests.

Many of these tests are related to the main "dimensions of personality", labelled "introversion" and "neuroticism" by Eysenck and co-workers. The report is confined to some 100 patients including those suffering from intractable pain, neurosis and epilepsy. A few psychotics were also included.

It was found that all operations involving the convexity—i.e., (1), (2), and (3)—are followed by the same pattern of changes in temperament and character. This pattern is characterized by decrease on five variables related to introversion and also decrease on five variables related to neuroticism. This pattern is consistent even if the neurosurgeon, the psychologist, the interval before re-testing, the diagnosis of the patients and the nationality and the mother-tongue are all different.

Whilst the *pattern* of personality changes remains the same after all the convexity operations, the extent of the change after the three different operations studied was directly related to the size of the lesion. The most pronounced changes in personality are found after the most extensive operation (standard lobotomy) and the least pronounced changes after the most circumscribed operation (excision of Brodman's Areas 9 and 10).

A different pattern of changes in temperament and character is found, however, after excisions involving the cingulate area (4). The differences are most striking on tests related to introversion. Operations in the orbital region (5) are also followed by a characteristic pattern of changes. This differs both from that following the convexity operations and that in the cingulate area.

The standard lobotomy (1) is the only one which is followed by a significant loss on intelligence tests (the Wechsler Bellevue scale and Porteus Mazes). This operation is also followed by an alteration in the use of language.

Evidence has been accumulated in our research indicating that the successful relief of the suffering of patients with intractable pain after frontal lobe operations is accompanied by decreased introversion. There are three groups of findings contributing to this evidence:

1. Decreases on five variables related to introversion follow upon operations involving the convexity whilst they do not occur after operations not involving the convexity. The former type of operation is clinically successful in relieving the suffering of patients with cancer and some other forms of pain; the latter is relatively unsuccessful.

2. Patients with pain who have been relieved by an extra-hemispheric operation resulting in a localized analgesia (e.g., trigeminal root section) do not show decreased introversion.

3. The patients with pain who improved most after frontal operations show greater changes on introversion variables than do those who improved least. The most improved patients also had the most introverted scores prior to the operation.

In addition, the successful relief of pain is accompanied by a decrease on variables related to neuroticism.

## SYMPOSIUM

### SOCIAL AND TECHNOLOGICAL ORGANIZATION IN INDUSTRIAL PRODUCTION SYSTEMS

### ORGANISATION SOCIALE ET TECHNOLOGIQUE DE LA PRODUCTION INDUSTRIELLE

*Chairman — Président*

WILLIAM LINE (*Toronto*)

*Discussion*

ALASTAIR HERON (*London*), ROSS STAGNER (*Illinois*),  
WILBERT E. MOORE (*Princeton*)

*Papers — Communications*

### A PSYCHOLOGICAL FOUNDATION FOR A MODIFIED THEORY OF MANAGEMENT

BY

RENSIS LIKERT (*Michigan*)

A number of studies in industrial organizations are yielding results which indicate that managerial and supervisory practices which deviate from traditional principles of management yield the highest levels of productivity, employee motivation and job satisfaction. These results will be examined along with other related research findings in an effort to discover why these deviations from traditional management theory yield superior results.

A cause is suggested, based upon these research findings, to account for the superior results obtained by these practices which deviate from traditional theory: the traditional theory of management is based on an assumption, which is no longer completely valid, as to the character of the motivational force which is most pervasive in influencing human behavior in working situations.

Another assumption, based on the research findings, is suggested as to

the character of the motivational force which exercises the most pervasive influence upon behavior in present day working situations. It appears that the deviant practices cited represent an application of this pervasive motivational force.

The author emphasizes the fact that in working situations a wide variety of motivational forces influence behavior. He also points out, however, that in developing an integrated theory of management it is necessary to use primarily one assumption as to the character of the most pervasive motivational force affecting human behavior in working situations.

### ASSESSMENT WITHIN VARIOUS FRAMES OF REFERENCE OF SOME FACTORS CONSIDERED AS CONTRIBUTORY TO INDUSTRIAL PRODUCTIVITY

BY

GUNNAR WESTERLUND (*Stockholm*)

A generally accepted definition of productivity in the work situation is the ratio between specified amount of a specified output, and *one or more* of the factors of input required to produce that output. A direct productivity measure is thus one of a series possible in a specified work situation, depending on whether output is compared to input of time, equipment, energy, money directly invested, effort, sacrifice of certain worker's satisfactions for others, and so on.

It must be recognized that the various direct productivity measures that can be used in one single work situation are not necessarily correlated, so that a desirable change, e.g., within management's frame of reference, in one is accompanied by a similar change in another. As a matter of fact, they may vary in opposite directions, when applied to the same situation. (This indicates deficiencies in the generally accepted definition of productivity.)

Another series of measures could be classified as more or less indirect productivity measures, e.g., absence figures, labor turnover, medical data on the workers, number of accidents, number of grievances. It is difficult to measure directly the amount of human energy expended in a situation, or satisfactions given up or found in a situation, and consequently the ratio output over these kinds of input can hardly be ascertained. Labor turnover and absence figures may be the first readily recognizable indicators of an undesirable development.

In this discussion I want to stress the fact that various measures of

productivity exist, which are applicable to the same work situation, and that the preference of one very often depends on the frame of reference in which the data are to be used. To exemplify, the economist tends to choose output per dollar of cost, the technician per unit of equipment, the motion study specialist per unit of time. Psychologists and sociologists would use still other measures of "satisfaction" to which output should be related.

This discussion of the various measures of productivity should be kept apart from selection among them as criteria to be used in validity studies—for instance in psychological testing. One may try to increase the reliability of a particular criterion of validity, while ignoring other direct and indirect measures of productivity.

A cursory reading of a number of textbooks shows dozens of uses of output per man-hour as a criterion. Twenty instances are enumerated in which some correlation between a change in the factor studied and output per unit of time has been found.

It seems to me permissible to formulate a hypothesis on this basis, saying that—in varying degree—each of these factors in every work situation does account for some of the variance in the productivity measure singled out to be used.

This emphasizes, I think, the necessity of two precautions in assessing factors in a work situation:

1. To increase the number of direct and indirect productivity measures applied, stating clearly the frame of reference for the investigation.
2. To make parallel studies of several factors in the same work situation.

## SOME CONTRASTING SOCIO-TECHNICAL SYSTEMS

BY

A. T. M. WILSON (*London*)

Some industrial production units—described in terms of technological lay-out and associated role-systems—will be contrasted, first, in attitudes of working groups to the production task; secondly, in differing types of role-system associated with the same technological lay-out; and thirdly, in differing levels of mechanisation, types of social organization and work-group culture for the same overall production task.

Small production units, particularly with a face-to-face group and a low level of mechanisation, usually contain within themselves a system of roles



which is adequate to all the manifest tasks of the unit and, not infrequently, meets certain latent needs of the work-group. With increased complexity of products, processes, group tasks and group size—of whatever origin—control and service functions may be developed as an external 'governing' system. With such differentiation, new problems of group relations may appear (e.g. conflicts between functional specialists and production executives). The resulting inefficiency may be gross in terms of almost any measure and yet be singularly persistent in character. Examples are given which raise questions of the part played by unrecognized differences in the conception of production systems and in running them.

The second type of contrast is exemplified by experimental modifications of 'traditional' role-systems in production units, following re-examination of the basic components in the unit's task. These modifications, in general, moved from the familiar practice of repetitive task-components undertaken by what were almost aggregates of operatives, towards structured work-groups with overlapping and complementary roles, increased group responsibility, internal leadership, and an altered relation to the surrounding 'governing' system.

Thirdly, examples are given, from the United Kingdom coal-mining industry, of role-systems and work-group cultures associated with different types and levels of mechanisation at the coal-face. These indicate the complexity of manifest and latent factors in the process of technological change.

From the descriptions given, questions were briefly raised concerning the potential contribution of role-system design, or re-design, to industrial practice; and to productivity (however estimated), to costs, to health, to job-satisfaction, and to safety. Many production unit role-systems appear to have been designed on intuitive assumptions, apparently derived from man-machine analogies, and often without full recognition of certain potentials of men, given certain work-group conditions. Principles of effective design need to take into account the overall production task, and the needs both of the production process and the working group.

Some urgent but more speculative questions are being raised by work in these fields. For example: assuming full agreement over technological change, in what general and what specific circumstances are difficulties of its implementation likely to be linked to unclarified factors, or inadequate principles, in the area of the associated role-systems?

## EFFECTIVENESS OF WORK TEAMS

BY

JOHN K. HEMPHILL (*Ohio State University*)

Although many changes have occurred in the social organization of work over the past century, the work team continues to be a significant unit in many industries. In fact, recent trends toward the decentralization of supervision and toward greater participation of the worker in management call for a renewed interest in methods for developing effective work teams. This paper has as its first purpose a description of recent trends in military research concerned with the effectiveness of such primary social units as the aircrew. It has the further purpose of suggesting how the concepts and findings of this military research may stimulate related research on industrial work teams.

Stemming from problems that developed out of the experiences of World War II, a basic research program on aircrew composition and training was initiated by the U. S. Air Force in 1950. A wide range of studies have been completed that have been focused on the following major problems:

1. If we are to achieve and maintain an optimal unit effectiveness, what variables must be considered in the assembly of new aircrews and in replacing members of established crews?
2. What experiences of crew members are associated with the development of desirable crew characteristics (e.g. high levels of crew coordination, high productivity, high morale, etc.)?
3. What types of crew leadership produce effective crews?
4. What patterns of crew interrelationships are associated with desirable crew characteristics? Is the homogeneity of the attributes of crew members a pertinent variable in determining crew compatibility? What arrangements in respect to status, prestige, or power should be fostered among crew members in order to achieve maximal productivity?
5. How do we determine the criteria by which we may evaluate the effectiveness of a work unit? What new criterion problems are introduced by concern with unit effectiveness that do not involve individual effectiveness, *per se*?

These and many additional problems have been investigated during the past four years of research sponsored by the military services. Unfortunately, few of the significant findings of these researches have been reported in the general literature.

Industrial work teams may be expected to have many characteristics in common with these aircrews. The problem of developing effective industrial work teams can be approached by using concepts and methods similar to those developed in these military studies. Future personnel research work in industry probably will direct increasing attention to methods for increasing the effectiveness of the primary work unit. This development will constitute a natural extension of current interest in better human relations.

## SYMPOSIUM

### RECENT TRENDS IN PERCEPTUAL THEORY

### NOUVELLES TENDANCES DE LA THÉORIE DE LA PERCEPTION

*Chairman — Président*

EDWIN G. BORING (*Harvard*)

*Discussion*

ALBERT WELLEK (*Mainz*), WOLFGANG METZGER (*Münster*),  
J. G. TAYLOR (*Capetown*), FRITZ HEIDER (*Kansas*)

*Papers — Communications*

### EXPERIMENTS WITH PROLONGED OPTICAL DISTORTIONS

BY

IVO KÖHLER (*University of Innsbrück*)

Classical psychophysics has not been able to cope with the perceptions of everyday life. Therefore the Gestalt-psychologists inserted a hypothetical process of organization between stimulus and perception. But is it necessary to solve the problem in this way? We have not as yet investigated to the full the world of stimuli in their mutual relations of space and time, and in their effectiveness for the whole array of sense receptors. What we need is statistical research on stimulation, not only on optical stimuli (they are not in themselves sufficient for visual perception) but on all the stimulus-effects present in a biological situation. Since we do not live in a world of contingencies but in a lawful world, this order must have its reflection also at the periphery of the organism in a kind of significant accumulation of certain coincidences and successions of stimuli. Our general question is whether such "stimuli-of-a-higher-degree", or stimulus-invariables, have a specific effect on the organism.

Of interest from this point of view are the results of prolonged experiments with mirror-glasses, prisms and other means of optically disturbing the retinal images, performed by Professor Erismann and myself in recent

years. I should like to deal with two problems arising from these experiments.

1. If the organism is able to find invariants of stimulation and adjusts to them, this is a preliminary step toward learning; it is "collecting statistics". It is possible, while wearing colored spectacles with different colors in the right and left halves of each glass, that the invariance in the stimulus situation is the coincidence of a certain muscular movement and a certain sensation of colour. Adaptation to these spectacles shows that unsurmountable barriers do not exist between the different modalities of our senses nor between sensory and motor activities.

Remarkable paradoxes observed in experiments with optical reversal also prove this. The paradoxes can only be understood if one gives up the purely optical standpoint and considers all sense-responses as a whole. In the experiments with devices which reverse the visual field left and right, it can be observed how seen subjects will gain a new "tone" of leftness or rightness if the subject is only able to reach them with the left or the right hand respectively. In spite of this, however, the purely *pictorial* impression remains reversed. For example it has been found after about two weeks of this experiment that the number "3" written by the subject in the normal way has its convexities on the right side but yet it remains reversed like the image in a mirror!

2. A complete "collection of statistics" requires not only the registration of the present stimulus patterns but also all former experiences. The "situational after-effect" is an example. This starts with our well-known phenomenon of adaptation together with the negative after-effect. This means that stimuli which normally correspond to a non-neutral sensation give a neutral sensation after a prolonged influence, and hence the originally neutral stimulus appears with a complementary sense-quality if the stimulated receptors are the same. And now: *This simple process can become conditioned to those stimulus-situations in which non-neutral stimuli have a significant frequency.*

Take for example blue rays of light always entering the eye when the gaze is to the left and yellow rays when the gaze is to the right. The process of adaptation will come to depend more and more upon this regular condition. This complicated stimulus presentation must be protracted for at least two or three weeks before the situational after-effect can clearly be observed. After the removal of such colored half-spectacles it then will occur. A formerly neutral gray color will appear yellow or blue *depending on whether the gaze is to the right or to the left*. This phenomenon is quite different from the usual after-image of color for it

shows surprising stability; it lasts for several weeks after the end of the experiment.

If these findings can be generalized (and results of the same kind have been found for space-perception and form-perception), then here we have the solution to the question mentioned above. Stimulus-invariables or stimuli of higher order are able to regulate sensibility in a multiple way.

## ORDINAL STIMULATION AND THE POSSIBILITY OF A GLOBAL PSYCHOPHYSICS

BY

JAMES J. GIBSON (*Cornell*)

The belief is advanced that a psychophysics of perception is possible if psychologists will undertake to reexamine the notion of a stimulus. We have been content to assert on the one hand that a stimulus consists of energy acting upon receptor-cells (the "proximal" stimulus) and on the other that a stimulus is an object, place, or event in the environment (the "distal" stimulus) but we have not undertaken to connect these two assertions. The psychophysical experiment, that is, the controlled systematic variation of *proximal* stimulation, has been successfully applied to the simpler variables of energy which are easily isolated, but it has seldom been applied to the more complex forms of stimulation which occur in daily life. These are the basic causes of our perceptions and the source of our knowledge of objects, places, and events.

We need a psychophysics of form, of surface, of depth, of motion, of deformation, and above all a psychophysics of the perceptual constancies. In all likelihood there exist during the concomitant multiple stimuli of daily life *invariants* of stimulation, that is, constant products in a mathematical sense, with which our constant percepts are in psychophysical correspondence. If we can achieve a psychophysics of object-constancy we shall be at least on the road to a psychophysics of meaning, of social objects, of symbols, pictures, and words. A psychophysics of this sort would be truly global.

The proximal stimuli for such percepts are clearly not variations of energy as such. The stimulation to which the percepts correspond must be differences in distribution, that is, differences in the adjacent and successive *order* of energy at a receptor-surface. These may be called *ordinal* variables of stimulation. Examples are given of several types of ordinal stimulation and of the qualities of phenomenal things which correspond to them.

This proposal is not so much a theory of perception as a strategy or a long-term program of research. Nevertheless it has theoretical implications. It suggests that the physiological explanation of perception (psychophysiology as distinguished from psychophysics) should be sought in ordinal *variables* of the neural process rather than in neural pictures or cortical replicas. It suggests also that whatever theory we adopt of the effect of memory on perception or of the formation of perceptual habits, we keep in mind the fact that perception is a kind of commerce with the world and that this is mediated by stimulation. Hallucinations, fantasy, and illusions are not the main problem for perceptual theory. Theories of the learning process in perception, therefore, should be able to account for the growth of sensitivity to the properties of the world. The value of hypothetical constructs like assimilation, expectation, habit, or attitude should be tested not merely by experiments on perceptual errors and distortions but also by experiments on the "effort after meaning" of which the errors are presumably a reflection. Postulates of this kind are not inconsistent with the possibility that subtle and intricate variables of stimulation are effective, even when the perception is most demonstrably learned.

## PERCEPTION AND VERBAL BEHAVIOR

BY

LEO POSTMAN (*California*)

The first part of the paper examines the operational status of concepts used in the analysis of perception. Two types of constructs are distinguished: (1) phenomenal constructs which attempt to specify what is perceived under given stimulus conditions, and (2) process constructs which embody hypotheses about neurological events intervening between stimulus and response. Frequently the same correlations between stimuli and responses furnish the basis for both phenomenal and process constructs. The problems that arise in the development of phenomenal constructs are discussed. First, there are serious methodological difficulties. The specific features of the percept ascribed to the organism may vary significantly with the specific responses used as pointer readings for the percept. The question of what it is the subject perceives in a given situation does not have a unique answer. It may, therefore, not be an appropriate question. Second, phenomenal constructs involve the fallacy of reification. Percepts, which are inferences from the stimulus-response

correlations, are treated as entities having specific content and form. It is argued that such reification of the constructs of perception is unwarranted.

The constructs of perception must be anchored not only in conditions of stimulation but also in the specific responses which are used to measure discrimination. Two such constructs are proposed: (a) perceptual habits, which are anchored to changes in stimulus conditions on the antecedent side and to a restricted class of responses on the consequent side; (b) perceptual response dispositions, which are higher-order constructs encompassing families of perceptual habits. This formulation emphasizes the continuity between the methods and concepts of learning and perception.

The second part of the paper surveys a series of experiments on the perceptual recognition of verbal stimuli, illustrating the communality of principles governing perceptual habits and verbal habits. (1) There is mounting experimental evidence that perceptual habits, like verbal habits, obey the principles of frequency and recency. (2) Associative context, recognized as an important determinant of verbal behavior, significantly influences speed of perceptual recognition. (3) Positive and negative transfer effects have been demonstrated for perceptual habits, in accordance with the principles established in experiments on verbal behavior. The generality of such transfer effects is attested by their occurrence across sense modalities as well as within sense modalities. Although the experimental evidence surveyed is restricted to the area of word recognition, it is argued that the operational logic holds equally well for other cases of perceptual discrimination.

## MEMORY EFFECTS IN PERCEPTION

BY

HANS WALLACH (*Swarthmore*)

In recent years experimental evidence for an influence of memory on form perception has been accumulating. Leeper's work with Street figures (1935) and the demonstration of a memory effect on the perception of three-dimensional form (Wallach, O'Connell and Neisser, 1953) are examples. An even more intimate relationship between memory and perception is demonstrated by those cases where memory influences perceived structure and where this, in turn, influences further perceptual processes. Examples of such complex perceptual functioning will be discussed, among others Ames' trapezoid window and Gibson's texture gradients.



# SYMPOSIUM

## MOTOR SKILLS

### L'HABILETÉ MOTRICE

*Chairman — Président*

ROBERT B. AMMONS (*Louisville*)

*Discussion*

Sir FREDERIC C. BARTLETT (*Cambridge*)

*Papers — Communications*

## FACILITATION AND INTERFERENCE IN THE PERFORMANCE OF A VARIETY OF PERCEPTUAL-MOTOR TASKS

BY

DON LEWIS (*State University of Iowa*)

Recent studies of facilitation and interference in the performance of perceptual-motor tasks have yielded diverse results. For example, under certain conditions of practice, proactive facilitation and proactive interference are manifested simultaneously during performance of the standard and reversed tasks on the Modified Mashburn Apparatus, and so are both retroactive facilitation and retroactive interference. In contrast, although interference effects are readily apparent with similar conditions of practice on the Two-Hand Coordinator, facilitative effects are difficult to detect. In pursuit rotor performance, proactive facilitation is a common occurrence while interfering effects seldom appear, except perhaps fleetingly. Facilitation is predominant during successive phases of practice on several tasks on the Star Discrimeter but interference effects are sometimes identifiable.

The diverse results obtained in studies of the facilitative effects of verbal pre-training on the subsequent performance of perceptual-motor tasks also suggest the major importance of underlying characteristics of tasks. Performance on the Star Discrimeter, for example, is markedly influenced by

different forms of verbal pre-training. In contrast, performance on the Mashburn apparatus appears to be impervious to verbal pre-training of the kinds typically employed.

A viewpoint to be presented in broad outline is that the findings on interference and facilitation (including the facilitative effects of verbal pre-training) are elucidated to an appreciable extent if perceptual-motor tasks are conceived of as falling at different points along a continuum extending from "predominantly perceptual" at one end to "predominantly motor" at the other. Predominantly perceptual tasks require principally the recognition and interpretation of stimulus cues (sometimes of intricate or obscure patterns of stimulation) and place minimal demands on strictly motor responses. The skillful performance of such tasks derives largely from the acquisition of interpretative proficiency. Predominantly motor tasks place maximal demands on skillful manipulation, and they can be performed well only after practice in manipulation has ensued. Manipulative proficiency is the principal component in the overall acquisition of skill. Many tasks, of course, lie between the extremes, and their mastery depends upon the acquisition of differing amounts of both interpretative and manipulative skill.

Interference is believed to arise primarily when skilled responses previously acquired in relation to one or more tasks are inappropriate to the performance of another task, while facilitation stems from interpretative and/or manipulative proficiencies which are compatible with the demands imposed by a new task. When verbal pre-training has a facilitative effect on subsequent performance, the task can be shown to depend more upon the acquisition of perceptual than of manipulative proficiency. Manipulative proficiency seems to develop relatively independently of verbalization.

## THE MOTOR FACTOR FROM PERCEPTION TO ACTION

BY

MARIO PONZO (*Rome*)

Several experiments on motor functions have been undertaken in Italy, most of them in the psychological laboratories of Turin and Rome.

Some of these investigations owe their origin to attempted improvements in the field of industrial psychology<sup>1</sup>; but, in due time, these researches

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<sup>1</sup> A great deal of this research was done in Italian trade-schools (Istituto Industriale Omar, Novara) with the collaboration of their director, Dr. Enrico Gatti.

were extended beyond their original field, towards the establishment of theoretical foundations.

In the course of experiments on weight-change thresholds it was observed that J.N.D. differed according to the procedure used in changing the weight. This variability of response was utilized in a number of experiments.

There are individual differences in variability of threshold under various objective and subjective conditions, and tests of this kind prove useful in developing machines with operating frictions adequate to the motor involvement in other phases of the same operation. In one of the experiments, a container was placed on a part of the body, and its weight was increased perceptibly by a slow inflow of liquid. The subject was not aware of the time at which the flow began, and the results obtained reflected the expectations of the subject rather than any constant psychophysical relations depending on stimulus variation.

In a similar experiment dealing with decreasing weights, some extremely interesting modifications of the body-scheme and of the position or shape of the objects used as stimuli were noted. When the container was held in the hand, instead of perceiving the weight-decrease which they were directed to report upon, the subjects perceived their arm as shortening, lengthening, bending, according to its position, and they also perceived a cone-shaped container as becoming smaller without being able to identify the reason for this strange impression.

A series of investigations was made using Gilbreth's photocyclographic technique. The task of the subject was to press very rapidly and always in the same direction three buttons placed in a triangular position. At a given signal he had to return to the preceding button and then resume the task in the original direction. Some subjects were able to perform quite well while others showed a breakdown of their motor pattern with a disorganization of behavior.

In another set of experiments conducted in the trade-schools, Gatti studied individual differences on the "reactive cycle". A light stimulus was presented in synchrony with each revolution of a lathe. The subject had to lift a lever as soon as the stimulus appeared. On the graphic record two factors could be computed:

- 1) the reaction time, or the time elapsed between the presentation of the stimulus and the response;
- 2) the recovery time, or the time necessary for the subject to ready himself for a new stimulus and a new reaction. It coincided with the period in which the lever was kept in the lifted position.

The variability between subjects was great but that within subjects was very small. It was proposed that this test be used for the selection of workers when a quick recovery time is needed.

In these investigations we are dealing with a central factor of psychic activity; in the study of motor skills this factor has to be analyzed, thus providing a method for the investigation of more fundamental psychic processes.

## GENERAL THEORY OF HUMAN MOTIONS

BY

KARL U. SMITH (*Wisconsin*)

Systematic ideas and related experimental procedures are being developed to integrate presently separated facts about human motions. The overall attack on the problem attempts to give some consistent meaning to work phenomena, maturational and embryological data, the neurophysiology of muscular coordination, and observed psychological aspects of human performance. The name, the theory of space-structured motions, has been used to designate our general descriptive approach to the field.

Although general performance studies of motion provide significant data about the applied problems of motor activity, theoretical study demands attention to the component movements defining the space structured motion pattern. Three fundamental component movements are typically involved in most classes of human motion. These components consist of postural, travel and manipulative movements, each of which has separate roles in the motion pattern. Component movement analysis of discrete motions, such as those widely used in industry, may be carried out accurately by means of electronic methods of motion analysis. Such methods provide, for the first time, adequate procedures for study of components of discrete motions in accordance with the demands of modern experimental design. In the analysis of precision of movement components in continuously controlled motion, as in tracking, recording and photographic methods are demanded.

Experimental study of the precision and duration of various types of human manual performances has confirmed the general assumptions concerning the movement organization of space structured motions. Such study has also clarified a number of related expectations about the role of handedness, of learning, and of perceptual discrimination in defining the characteristics of the component movements in the motion pattern.

The conception of complex motion as a series of space structured movement components has led to a series of investigations dealing with the physical reactive dimensions of motions. Data have been obtained that provide the means of appraising scientifically some current concepts of time and motion study. Such studies have also suggested new approaches to some problems of human engineering, especially the evaluation of the instrumental factors in visual tracking. The general point of view outlined here served as the background for the prediction and later experimental proof that aiding devices in visual pursuit tracking are actually hampering devices.

In relation to the general theory of space structured movement, interesting observations may be made on the evolution of skilled motion, the separated stages of motor development in maturation, and the general experimental results bearing upon the nature of cortical function in motion.

## TIMING AND ORGANIZATION OF MANUAL SKILLED PERFORMANCE

BY

A. T. WELFORD (*Cambridge*)

An outline was given of some recent experiments on motor skill which were done primarily with a view to understanding the limitations on performance that arise in middle and old age. In the course of this work it was necessary to consider some of the fundamental problems of the nature and organization of motor performance, and the experimental results appear to have implications beyond the problem of ageing. Analysis by means of detailed recording of the form, timing, order, etc., of the component parts of serial performances was attempted with a view to studying various functions which contribute to the performance, without removing them from their "context".

Recent experiments by Singleton (1953, 1954) using a serial reaction task have indicated that central factors play a greater part than has hitherto been supposed in determining the speed of performance and its changes with fatigue and age. These experiments link with the work of Seymour (1954) and of Wehrkamp and Smith (1952) which indicate that manipulation rather than movement times change with voluntary alteration of speed and with practice.

Studies of simple repetitive performance contribute to the understanding

of skill, yet they do not illustrate the essential characteristics of "skill" in any ordinary sense of the word. Experiments are required on sequential, co-ordinated performance and three lines are suggested which seem worth pursuing:

(a) Requirements for speed and accuracy in one part of a cycle of operations can determine performance in another part where a different balance would be more appropriate. Indications of this have been obtained in industrial studies, and experimentally by Wehrkamp and Smith (1952) and by N. T. Welford (unpublished).

(b) A re-examination of experiments by Craik, Vince and others on serial reaction times led to the suggestion (Welford 1952) that there is a "single-channel mechanism" in the central processes involved in the perception of incoming signals and the forming of action in response to them. The central processes taking place during a reaction time can overlap with the execution of a preceding movement, but not with another reaction time. The signals which occupy the central mechanisms are not only those which come from the display but the sense data arising from the responding movements. These latter can in certain circumstances be neglected and this leads to increased speed of performance together with an uncanny absence of conscious awareness of what is being done. Taking these experiments in conjunction with those of Hick (1952) and Crossman (1953) which show reaction times for any class of signal to be related to the degree of choice or uncertainty or accuracy required, we have the beginnings of a method for calculating maximum speeds of performance at serial tasks.

(c) Perhaps the most important problems of skill lie in the maintenance of order among sub-units of performance, and the size of unit that can be maintained. Some striking evidence of the breakdown of performance as the number of sub-units is increased has been obtained recently by Kay (1954).

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## DEVELOPMENT OF VOLUNTARY MOVEMENTS

BY

A. B. ZAPOROZHETS (*Moscow*)

Human voluntary movements are learned. According to Pavlov they develop as a result of the formation of conditioned reflexes. What is the nature and role of psychical activity taking place during the formation and the use of motor habits and how does this activity proceed and change at different stages in the child's development?

In our research, we studied the development of voluntary behaviour of children from 2 to 7 years of age. In one series of experiments, we made a study of a system of movements by which children had to press on reaction keys in a definite sequence; in the second type of experiments, children developed the habit of finding their way in a labyrinth; in the third type, children had to learn an elementary system of gymnastic exercises, etc.

As a result of these experiments the following facts were established.

At the early stages of the child's development, the orienting activity preliminary to the performance of the proper working, efficient movements which is in fact a study of the situation is carried out in the form of external activity with objects. It consists of peculiar acts called by Pavlov orienting reflexes or "what it is" reflexes. These reflexes are such as turning of the head to appearing stimuli, touching new objects with the hand, following them with the eyes, etc.

By such investigating activities, the child acquires a preliminary knowledge of the situation before he begins to practically act in it. The stimuli found in the process of orientation bring about the reactivation of the tracks of former experience in new combinations which more or less reflect the terms of the task and can, to some extent, predetermine the performance of movements to follow.

Experiments have shown (T. V. Endovitskaya, J. Z. Neverovich, L. S. Tsvetkova) that conditions favourable for the development of the orienting activity facilitate the use of mastered movements and the acquirement of new knacks and habits.

It was found, in the process of further research (T. V. Endovitskaya, J. Z. Neverovich, O. V. Ovchinnikova), that the character of the orienting activity in the formation and use of movements changes considerably during the process of the child's development.

Motor tactile orientations are initial forms in younger children. At this

stage the eye follows the movement of the hand. However, later on the eye, learning from the experience of the hand, is able to perform the orienting function by itself. For example, sometimes, elder children, after following the labyrinth with the eye, can more or less precisely perform the necessary movements.

In the process of the child's development an ever increasing role in the organization of his movements is played by speech which is the second signal system. According to our studies, the word performs this function by causing orienting reactions to the objects named. At this stage, the word can orient only in the present, directly perceived situation. Later on, the accumulated experience results in the child's forming firmly established systems of orienting reflexes corresponding to definite objects. These systems get connected with the word and undergo a specific abbreviation and generalization; now, the word can cause orienting "in mind", i.e., conditions which are not perceived directly at the present moment.

In this case voluntary movements acquire, in fact, the form in which they have been usually described in psychology, i.e., movements performed according to the representation of the goal, the means to reach it arising spontaneously in the mind.

Thus, our research shows that voluntary or consciously regulated movements are a product of development in the course of which initial forms of the externally developed orienting activity are gradually reduced and acquire the character of the internal psychic processes which precede and outline the way to motor reactions to follow.



FRIDAY JUNE 11 — VENDREDI LE 11 JUIN

## SYMPOSIUM

### PERSONAL CONTACT AND CHANGE IN INTERGROUP ATTITUDES

#### INFLUENCE DES CONTACTS PERSONNELS SUR LES CHANGEMENTS D'ATTITUDES « INTER-GROUPES »

*Chairman — Président*

M. BREWSTER SMITH (*New York*)

*Discussion*

CARL I. HOVLAND (*Yale*), THEODORE M. NEWCOMB (*Michigan*)

*Papers — Communications*

#### ADJUSTMENT IN A FOREIGN SOCIETY: NORWEGIAN FULBRIGHT GRANTEES VISITING THE UNITED STATES

BY

SVERRE LYSGAARD (*Oslo*)

In March, 1953, about 200 Norwegian Fulbright grantees were interviewed about their experiences in connection with a stay in the United States. The present paper is not a "summary" of this study; it concentrates on *adjustment* problems in particular. Adjustment is analyzed from two points of view: a) the relationship between different items of adjustment and b) adjustment as a time process.

There seems to be a strong tendency to "generalize" adjustment: different items of adjustment are markedly interrelated. The pattern of generalization is most pronounced between items of the same general "area" ("professional-educational" or "personal-social"). But association

exists also (less pronounced) between pairs of items belonging to different areas.

Such a "generalization" does not seem to be due to any uniformity in the relative magnitude of the obstacles set by the American milieu in respect of the different items considered. Nor does it seem to be due to any general inclination, on the part of the respondents, to express "like" or "dislike", "adjustment" or "maladjustment", independently of the specific situations to which reference is made. It is concluded that "generalization" is an effect of *interplay between person and situation*. Good adjustment on one item may increase the general feeling of security in the foreign milieu and thus encourage the individual to engage successfully also in respect of other items. Adjustment may follow the "principle of cumulation": it starts moving in one direction if it is only given a push in that direction somewhere along its chain of interconnected elements.

As a *process over time*, adjustment seems to follow a "U-shaped" curve: to begin with, adjustment is felt to be good; then comes a period in which adjustment is not so good; later, however, adjustment is again felt to be good. This adjustment "process" is interpreted thus:

To begin with, the grantees are content with the novelty of things, they are happy that they are so able to "get along", they get pleasure out of accidental and superficial "contacts". After some time, however, the "adventurous" features of "being abroad" lose their appeal and a need makes itself felt for more intimate and warm friendships, for more profound involvement of the total personality. This need may not find its satisfaction right away, however, and a feeling of "loneliness" may evolve. This feeling may be accompanied by "projection" phenomena: one blames the society one visits for not providing human contact, etc. This unhappy state of affairs is gradually being ameliorated. One becomes integrated in social groups and feels more like a regular member of the community.

### SOME FACTORS WHICH INFLUENCE THE ATTITUDINAL OUTCOMES OF PERSONAL CONTACT

BY

STUART W. COOK and CLAIRE SELLTIZ (*New York University*)

During the past few decades there has been in the United States an increasing amount of research on intergroup relations and on the effects of direct contact between members of different groups. The present paper will present briefly what seem to be some of the basic variables involved

and will consider their implications for the understanding of cross-cultural contact.

In attempting to uncover the effects of intergroup contact, we must distinguish between the *situation* which provides the opportunity for interaction and the *interaction* which actually occurs. We need also to differentiate among different types of interaction, types of situation, and types of effect. Analysis of the variables, therefore, will be organized in three categories: *characteristics of the contact situation*, *characteristics of the individuals involved*, and the *attitudinal and behavioral outcomes*. A fourth category which we can only suggest at this time is *the interaction which occurs* within a given situation.

Among the characteristics of the contact situation requiring specification, such as its extent in time and the degree to which it requires individuals differ from commonly-held unfavorable stereotypes about the are its "acquaintance potential", the opportunities it provides for "getting to know" another person; and its "social acceptance implication", or the extent to which interaction with another in the situation implies that one is willing to accept him as a social equal and, at least potentially, as a friend. These two variables seem to underlie the characteristic of interaction loosely referred to as "quality" or "intimacy".

The individuals in the contact situation can be thought of as both object and subject. As objects seen by the other group, the extent to which the interdependence of action, two are discussed by way of illustration. These object-group, and the extent to which they resemble the subject-individuals in interests, experiences, etc., will have important bearing on the outcomes of contact experiences. As subjects, the nature and intensity of initial attitudes as well as personality characteristics which predispose one to ethnocentrism will affect differential reactions to the same situation.

Change itself can be broken down into a number of dimensions. Thus there may be change in beliefs, in feelings, in policy orientation, and in behavior. Moreover, these different kinds of change can be subdivided in terms of their specificity or generality. In general, studies measuring change specific to the individuals in the situation or to a particular type of situation have found interpersonal contact and favorable change on all four dimensions closely related. Studies measuring generalized feeling and/or policy orientation, however, have suggested that such change occurs only in situations characterized by high acquaintance potential, high social acceptance implication, and by group norms favoring intergroup association.

Though major differences between intergroup contact within a country

and cross-cultural contact exist, they would seem rather to necessitate a broadening of the determinants of attitude change and of the range of attitudinal objects considered, than to invalidate the categorization of variables important in studies within the United States.

## PERSONAL CONTACT IN SCHOOL AND CHANGE IN INTERGROUP ATTITUDES

BY

H. E. O. JAMES (*University of London*)

Reassuring, friendly, and enjoyable contacts between members of different ethnic groups lead not only to friendly attitudes between the persons concerned but also to favorable attitudes towards their ethnic groups. Similarly disturbing, unsociable, and hostile contacts lead not only to unfriendly interpersonal attitudes but also to unfriendly intergroup attitudes.

The first of these two hypotheses was tried out in an experiment in which African women teachers took charge of English school classes. Everything possible was done to insure good relations between teachers and pupils. Observation and interviews showed that relations were good. Ratings of attitudes towards Africans and analysis of interview content showed a significant improvement in attitudes during the experimental period and no change during corresponding control periods.

The African teachers were regarded by the children both as persons and as Africans. Subordination of either aspect reduces the chance of changing attitudes. Overshadowing of the African aspect by the personal may build up an attitude to a person without affecting African attitudes; its over-emphasis favors responses in terms of existing attitudes only.

Attitudes to Africans were changed by the development of an attitude of liking for an African person. This prompted revision of the old attitude and supplied a model and motivation for the new.

Personal contact is both exploratory and emotional. Children, faced with newcomers, need reassurance against fears, assurance of security, and promise of friendliness and enjoyment; all are needs disposing of powerful emotions and motives. Children think that they can find out from the newcomer's responses to themselves by word, look, and act, what sort of person they are dealing with and whether a person of different ethnic group is fundamentally the same as themselves.

Hence the many items in the interviews concerned with fear and danger

and reassurance against them; with sociability, friendliness, kindness, and other items promising security and enjoyment; and with the routine of daily life and other indications that they are dealing with persons of the same kind as themselves.

The new attitudes were stereotypes in that the same description and emotional attitude was applied to all Africans. Being favorable, these would lead to new contacts, not avoidance of them; being free from gross misconception and based on firsthand experience, to sounder and friendlier thinking about Africans than before.

The experiment was carried out in a school and community where role relations between blacks and whites and between teachers and pupils did not make good personal contacts too difficult to achieve. Furthermore, the children's contacts with Africans had been slight and few before the experiment.

Two favoring but not indispensable circumstances were: that the Africans were women; there is evidence that men would have been more disturbing. And that the teacher-pupil interaction took place in the class group with much subsequent informal discussion, the reaching of a consensus, and group endorsement.

## SOME SPECIAL PROBLEMS OF LEARNING AND TEACHING PROCESS IN CROSS-CULTURAL EDUCATION

BY

RONALD LIPPITT and JEANNE WATSON (*Michigan*)

Twenty-nine German visitors were studied intensively, by interviews and projective tests. Measurements were made at the beginning of the visit, before departure, at the end of the year, and again eight months after returning home. The data included values and attitudes about the United States and Americans; about Germany and Germans; about international relations; democracy; the initiation and process of social change; relationships of the person with his home culture; ideas about family life, sex roles, peer relations, and relations with authority; and also about frustrations and satisfactions of the visit in the United States, and after return home.

This report does not summarize the statistical data on change and non-change of values and attitudes. Instead we are discussing five problems of interaction and learning which were important for the visitors and the hosts. These five problems are:

1. The problem of achieving personal security and self-esteem in relation to the host culture and its representatives.
2. The problem of ambivalence about responsibility, authority, and autonomy in the activities of the educational program.
3. The problem of transforming differences between the home culture and the host culture into learning experiences rather than alienation and withdrawal experiences.
4. The problem of maintaining appropriate cognitive and emotional relationship with the home country while in the host country.
5. The problem of maintaining and using new learnings in the home country after return.

Finally, we point out that the representatives of the host culture need to do a lot of work on learning the difficult multiple roles of host, teacher, friend. Exchange programs may have negative or minimal effects unless they are carefully planned and humbly executed.

## SYMPOSIUM

EUROPEAN CHARACTEROLOGY

CARACTÉROLOGIE EUROPÉENNE

*Chairman — Président*

HELMUT VON BRACKEN (*Braunschweig*)

*Commentator — Commentateur*

GORDON W. ALLPORT (*Harvard*)

*Discussion*

HAROLD H. ANDERSON (*Michigan*), HANS J. EYSENCK (*London*),  
ALBIN R. GILBERT (*Wheaton*), TORSTEN HUSEN (*Stockholm*),  
WOLFGANG LUTHE (*Montreal*), A. A. ROBACK (*Emerson College, Boston*),  
ALBERT WELLEK (*Mainz*), WERNER WOLFF (*Bard College*),  
FREDERIC WYATT (*Michigan*)

*Papers — Communications*

(The papers given at this symposium will be published together with additional articles and commentaries under the title *Current Perspectives in Personality Theory* (Basic Books, New York 1955) under the joint editorship of H. P. DAVID and H. V. BRACKEN).

(Les communications présentées à ce symposium seront publiées avec d'autres articles et commentaires, sous le titre *Current Perspectives in Personality Theory* (Basic Books, New York 1955) par les éditeurs suivants : H. P. DAVID et H. V. BRACKEN).

### THE PRESENT SITUATION OF GERMAN CHARACTEROLOGY

BY

HELMUT VON BRACKEN (*Braunschweig*)

Surveying the history of personality psychology in recent decades we find that despite national peculiarities of thought there is nevertheless a core of agreement. One common feature is the degree to which diagnostic testing has gained in significance everywhere. Since the beginning of this

century diagnosis has steadily gained ground. Earlier advances, of course, are not entirely superseded; but the clinical point of view, and the needs of clinical psychology have everywhere shown the greatest progress.

Clinical and diagnostic practice need theoretical concepts. "Percepts without concepts are blind", wrote the German philosopher Kant, just as "concepts without percepts are hollow". The concepts we need are provided by characterology or, as the Anglo-Americans prefer to call it, personality theory.

In *Germany* characterology is an important focus within the broad field of psychology. It is at least as important as the psychology of subjective mental processes. In recent decades the nature of characterology has markedly changed. Its pronouncements are now being tested as never before in the light of practical diagnostic experience. This fact has led to a decrease in interest in typologies that were so popular in Germany during the period between 1920 and 1938. Individuals are now regarded as more diverse in nature than coarse typological schemes allowed. Furthermore the characterology of Ludwig Klages has lost ground. Klages' characterology is based essentially on Nietzsche's philosophy. This anti-rationalism expressed itself earlier in strong interest in expressive movements. Anti-rationalism is held in less esteem today, and correspondingly we can no longer regard expressive movement as a dominant interest, as it was around in the 1930's. Since that time, the theory of levels (*Schichtentheorie*) or, as you might say, the theory of stratification, has come to the fore, a trend dominating since 1938 when important works on the levels or strata of personality were published by Lersch and by Rothacker.

'Schichten' theory is often criticized as being too rigid in its conception of personality organization; typologies also. Since personality change plays an important role in human life, it cannot be adequately accounted for by these theories. Take for example the findings of one of my young Braunschweig colleagues, Thielitz, in testing apprentices in a steel plant by the Rorschach method. Fatigue, it turned out, causes not only peripheral personality changes, but causes comparatively easily a disintegration of the personality. In this way German characterologists put more emphasis on personality change, on what I call the kinetics of personality.

By way of summary: To my mind German characterology of today is the product of certain movements. It is no longer a matter of pure armchair endeavors, but is closely connected with clinical psychology. It stresses the problems of the organization of personality and puts more emphasis on the kinetics of personality.

In this way German characterology shows growing independence from



philosophy; it is becoming more and more identical with personality theory. When you look closely at these movements you will find a certain convergence of interest and way of thinking in the personality theories of different countries. This convergence is happily bringing together psychological science, and preparing the way for better mutual understanding of nations.

CARACTÉROLOGIE ET ÉTUDE EXPÉRIMENTALE  
DU CARACTÈRE  
(SITUATION ACTUELLE EN FRANCE)

PAR

RENÉ ZAZZO (*Paris*)

On observe en France, depuis la fin de la seconde guerre mondiale, un intérêt nouveau pour tous les problèmes qui touchent aux aspects affectifs des comportements humains et à la formation du caractère.

Cet intérêt se manifeste à la fois par le regain de la psychanalyse, par la pratique intensive des épreuves de personnalité (techniques projectives, questionnaires, tests d'analyse factorielle), par le succès de la caractérologie de R. Le Senne, par l'approfondissement des travaux d'Henri Wallon.

La systématisation caractérologique, jusqu'alors assez étrangère à l'esprit de la psychologie française, apparaît dans l'œuvre de Le Senne et de ses disciples qui ne sont d'ailleurs pas psychologues de formation mais moralistes, pédagogues, philosophes. La caractérologie de Le Senne emprunte à Heymans et Wiersma la notion des trois propriétés constitutives (émotivité, retentissement primaire ou secondaire) dont la combinaison conduit à la schématisation de huit types fondamentaux. La contribution originale de Le Senne réside sans doute dans la distinction radicale qu'il établit entre caractère, personnalité et moi.

— Le caractère est l'ensemble immuable des dispositions héréditaires qui constitue le squelette mental de l'homme.

— La personnalité est la totalité historique et concrète du moi.

— Le moi est le centre actif, libre, qui sur la base du caractère crée la personnalité.

La plupart des psychologues, et notamment les deux maîtres actuels de la psychologie française, H. Piéron et H. Wallon, refusent une telle systématisation qui opère, selon eux, à partir de postulats philosophiques, une dissociation injustifiée dans l'unité de la psychologie humaine.

L'étude du caractère, telle qu'elle fut entreprise par Wallon depuis bientôt trente ans donne une importance primordiale à l'émotion qui, dès les

premiers mois de la vie, réalise une sorte de symbiose entre l'enfant et son entourage. L'étoffe primitive de l'émotion est faite des fluctuations de la tonicité, d'attitudes motrices variables d'un individu à l'autre. Ainsi Wallon parvient à la description de types psycho-moteurs qui sont aux origines du caractère. Mais Wallon rejette cependant une conception organiciste du caractère. Les réactions et notamment les réactions émotives ne peuvent être envisagées indépendamment du milieu qui les provoque. L'émotion opère dès ses premières manifestations une fusion du biologique et du social. L'être humain, dit-il, est un être social non par suite de contingences extérieures : il l'est génétiquement.

La conscience et le moi n'apparaissent pas comme des principes explicatifs, étrangers à la science, mais comme les effets mêmes de l'ontogénèse. Effets qui deviennent causes à leur tour dans la conquête de la liberté et de l'autonomie.

## LA RÈGLE DE CONCORDANCE EN CARACTÉROLOGIE

PAR

RENÉ LE SENNE (*Paris*)

L'expérience de la diversité humaine a conduit en Europe à une grande variété de caractérologies. L'unification systématique de la caractérologie doit se faire par l'application d'une règle que nous appellerons la *règle de concordance* : elle consiste essentiellement à dégager les résultats communs qui ont été « vus », confusément ou distinctement, par divers caractérologues, puis à construire la caractérologie, au sens le plus large du mot, en procédant de ces premiers résultats inductifs vers d'autres, qui les prolongent, les précisent et les complètent, comme par exemple une concordance des Evangiles synoptiques permet d'en dégager les éléments communs.

Cette règle doit jouer à deux étages. Suivant la règle de concordance *restreinte*, on confrontera les diverses caractérologies. Suivant la règle de concordance *généralisée*, ce sont les diverses disciplines anthropologiques qui doivent venir chercher leur confirmation mutuelle en concourant dans la caractérologie.

## LE FONDEMENT DYNAMIQUE DE L'EXISTENCE FÉMININE

PAR

FRÉDÉRIC J. J. BUYTENDIJK (*Utrecht*)

Nous entendons par le fondement dynamique, *la manière* d'exécuter les mouvements, en tant qu'elle n'est pas déterminée par la situation, mais qu'elle est la manifestation d'un style moteur incarné dans l'individu. Par

l'existence féminine nous entendons, la *manière* dont la femme se trouve engagée corporellement au monde ; la *manière* dont elle est consciente (surtout dans l'irréflexion) de ses propriétés physiques, ses dispositions fonctionnelles, son apparence, ainsi que du monde, d'autrui et d'elle-même comme un réseau de signification constituées et assumées.

On sait que la forme phénoménale du dynamisme et de l'existence de la femme dépend de la structure sociale, des normes culturelles proposées à l'éducation et de l'interprétation traditionnelle des différences entre les sexes telles qu'elles s'établissent par la pratique des rapports entre l'homme et la femme.

On peut constater déjà chez l'enfant âgé de moins d'un an des caractéristiques masculines ou féminines, tout particulièrement de la motricité. On reconnaît le garçon *en général* à la prépondérance des mouvements brusques, saccadés, énergiques, expansifs, même agressifs. Les mouvements de la fille sont plus souples, coulants, harmonieux, onduleux. Cette différence dynamique est conditionnée par la constitution corporelle, qui détermine également les différences morphologiques, physiologiques et la physiognomie. Il est évident que la description des propriétés de la motricité ne peut se limiter à ses aspects quantitatifs, c'est-à-dire objectifs au sens de la physique. En fait, l'essentiel est la *signification humaine* de la typologie dynamique. Celle-ci, en effet, implique une multitude de schèmes régulateurs pour les relations intersubjectives et intersexuelles ou même pour toutes les relations du sujet au monde. L'analyse de ces schèmes moteurs ne se fondant que sur le sens du comportement, laisse entièrement de côté l'examen des impressions subjectives de la femme, ainsi que toute théorie spéculative sur la structure de l'« intériorité », la fonction des tendances et des besoins, la portée de motivations.

La fillette, étant dès sa naissance présente *par son corps et dans son corps* d'une autre manière que le petit garçon, rencontre les objets et surtout sa mère d'une autre façon que celui-ci. Dès ses premières expériences le garçon forme un monde de résistance, d'opposition, d'obstacle, qui réclame sans cesse de nouveaux mouvements expansifs. La fille, par son dynamisme adaptatif, est plutôt orientée vers un monde de co-existence fait de la qualité et de formes sensorielles qui suscitent, de leur côté, des mouvements adaptatifs. *Gesell* remarque à juste titre : « Girls dress themselves much more efficiently and earlier than boys, due to a better fine motor co-ordination and especially a more flexible rotation at the wrist ». (*The First Five Years of Life* : London, Methuen, p. 248.)

Il n'y a pas d'opposition radicale entre les sexes. Les deux modes d'existence humaine peuvent s'incarner dans l'un et l'autre sexes. En tout

cas, le fondement dynamique est à chaque âge renforcé ou refoulé par l'éducation, qui commence déjà dans les premiers mois de la vie par la répétition des soins maternels qui engendrent traditions et habitudes, et par la mise en valeur de la mobilité enfantine.

Le schéma du dynamisme féminin se montre dans les jeux des enfants et il explique les propriétés psychiques constatées dans plusieurs travaux. La différence originelle des sexes se réalise de la façon la plus prononcée dans le contraste du travail et du souci. L'acte du travail vise un monde de résistance à vaincre. Le travail est toujours prestation, création, production de formes fondées sur la volonté de domination. L'acte du souci s'accomplit par un dynamisme adaptif et une conscience de valeurs non créées mais découvertes et qu'il faut, dans l'expérience d'une co-existence, conserver, susciter ou augmenter.

## THE PROBLEM OF CHARACTER CHANGE

BY

HANS THOMAE (*Erlangen*)

1. *Definitions:* Character—as the structural aspect of personality—is standing for all internal conditions of overt behavior being constant to a certain extent. But character as well as personality, structural conditions of behavior as well as actual behavior should be regarded as processes of different speed. Character, then, would be a process of a very slow type, behavior a process of very fast type.

Character change means a difference between two or more stages of a process called 'character'.

2. *Evaluation of research methods:* If character change is to be defined by the differences in two stages or qualities of the same process one cannot trace it without following up a sample of personality processes or life-histories. So the psychological biography, the case-history and the longitudinal study belong to the most important in our field. They can be applied if the codification of principles for a scientifically useful biography started by Dollard, G. W. Allport, and others is being observed by workers in character change research.

3. *Channels of character change:* Objectivation (i.e. reduction of emotionality, "Versachlichung"), quantitative and qualitative modification in the individual's orientation with regard to his life-situation, variations of personality themes and techniques of life, variations in intensity or extensity of emotional life, growth or decline of readiness to response in

general, to emotional response especially, of control of behavior, of intra or extraversion, of identification with regard to persons and values are only some of the channels in which character change takes place. There is a great variety of these channels of which we do not know very much as yet. But we never will learn very much about them without intensive use of the experiences of clinical work and the use of field-workers getting biographical material for us out of real life-situations.

4. *Outline of a research program:* Like in many other fields, in psychology we must start from the very beginning if we want to make real progress in character change research. At first, a great deal of phenomenological research has to be done. All forms of transitions or character changes happening in real life-situations must be registered. Then experimental or testing situations can be devised and longitudinal studies started at a more advanced level. At this point, other problems which have come too soon into discussion up to now, can be studied. To these belongs the problem of the causation of such changes (internal vs. external stimulation), the problem of prediction of time and direction of character change, and so on.

Ideas like these are dominant in the following research projects in which the author is participating or which he is guiding:

- a) a longitudinal study on German children in six different centres, combining both interview, testing, and observational methods;
- b) a systematic collection of psychological biographies of people with a clearly defined background (religious, liberal, socialist, "Wandervogel") by specially trained field-workers;
- c) statistical and biographic research in the changes of adult life between 20—50.

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**SYMPOSIUM**  
**INFORMATION THEORY IN PSYCHOLOGY**  
**THÉORIES D'INFORMATION EN PSYCHOLOGIE**

*Chairman — Président*  
**J. C. R. LICKLIDER (M.I.T.)**

*Discussion*  
**PAUL M. FITTS (Ohio State), FREDERIC C. FRICK (Washington),**  
**HENRY QUASTLER (Illinois), CHRISTOPHER POULTON (Cambridge)**

*Papers — Communications*

**STATISTICS, STRUCTURE AND STRATEGY**

BY  
**E. B. NEWMAN (Harvard)**

The purpose of this paper is to review some of the applications of information theory to psychological problems, and to discuss briefly the interrelations among them.

Information is a statistical concept. It assumes at the outset some set of elements; they may be objects or events, or messages or symbols. Possession of one of the elements in this set reduces the uncertainty which exists prior to its reception. The reduction in uncertainty is a measure of the information conveyed by the element in question.

Three possible models are distinguished. The first of these is the transmission case. There is a source of information, a channel by which the information is transmitted, and a receiver of information. Noise, or error, in the channel introduces some degree of equivocation about the message transmitted, thus reducing the capacity of the channel to transmit information.

The second model analyzes the information in a sequence of elements. If each element is independent of the others it contains the maximum information. But commonly there are constraints imposed by the pattern

in the sequence. To this extent each element may convey less information. In the case of language this is thought of as redundancy. In more conventional materials it may be considered as "meaningfulness", while in other material such constraints are termed more neutrally as pattern.

The third model is an attempt to express the more general structure of the second model. Obviously any sub-set of elements may be examined to see whether they occur in conjunction with any other sub-set. Speaking more conventionally, the occurrence of one set of items may be correlated with occurrence of a second set of items. Information measures give us an extremely general test of this possible relationship. It then becomes an empirical question whether one can discover the most significant of such relations. The problem is much like that of discovering simple structure, in factor analysis. No general rule of procedure is evident except for intuitive ordering in the attempt to find general structure.

Each of these models has a number of psychological applications. Psychophysics may be regarded as an instance of the transmission of information. Sequential structure is already well known in many types of learning. The most fruitful application of the more general model would seem to be visual perception. In all cases it should be emphasized that a model plays an ancillary and supporting role to the substantive analysis of the problem.

## INFORMATION, ASSOCIATION AND INTERACTION

BY

WILLIAM J. MCGILL (*M.I.T.*)

There are some points of similarity between a human being and a communication channel. For example, there are inputs or stimuli that go into the human, and outputs or responses that come out of the human. It is not difficult to imagine a function that maps the stimuli into the response so that, in this elementary way at least, communication seems to go on.

A closer look, however, at some of the problems of trying to get information through a human, convinces us that if humans act like communication channels, they are certainly complicated channels. We can see this when we try to apply Shannon's model of transmitted information to describe information transmission through a human channel.

We may begin by supposing that any unpredictability in responses is due to random noise. This is essentially the picture of information trans-

mission proposed by Shannon. With this start we now perform an experiment in which we measure the probabilities of the various outputs from the human operator for any given input to the human operator. This experiment should tell us how much of the input information gets through the human, and how much is lost in the noise.

Since we are psychologists, however, we are sure to be confused about what part of the output of a human being is *random* noise. We certainly do not know how to recognize response-noise when we see it. The operator's responses are affected by many variables, not just the stimuli we put into him. Consequently, responses that appear unpredictable may be explained very easily once we know all the variables that are operating.

In communication terms, the experimental problem that faces psychologists is noise analysis. We must try to discover how many different transmitters are operating to influence the output of the human being. If only one of these transmitters is studied, the effects of all the other transmitters will look like noise. But the noise will not be random. It will be only partly random. Our job is to identify and measure the non-random parts of the noise.

It is only a step further along this line of argument to show that psychological applications of information transmission require a multivariate extension of Shannon's model of transmitted information. The results of one such extension are discussed in this paper. Two results are especially interesting: (1) multivariate information transmission is a close relative of the analysis of variance in classical statistics; (2) knowledge of the signal output of one transmitter may, in some cases, actually reduce the amount of information transmitted from another transmitter. This is a reversal of the familiar verbal rule relating knowledge and information. It means essentially that in higher order information transmission, negative information may occur.

## SUPRA-LOGICAL BEHAVIOR IN AUTOMATA

BY

D. M. MacKAY (*University of London*)

This paper is concerned with the behaviour possible in an automaton intended explicitly as a hypothetical model for comparison with the human information-handling system. It is not concerned with the mere imitation of human performance by a suitably programmed universal computer; this is already demonstrable to have no limits in principle. The object is rather to make some suggestions towards the design of a realistic informa-



tion-flow model that may serve as a tool of research (a) by offering a common language in which physical and psychological concepts may fruitfully be related and (b) by giving rise to hypotheses on which both psychological and physical investigations may be brought to bear.

After a brief summary of the basic requirements for goal-guided (as distinct from goal-oriented) behaviour, the principal question is introduced: How far is it possible to envisage an automaton that might reasonably reproduce such "supra-logical" human activities as the invention of fruitful hypotheses, the imagination of fictitious situations, and the like?

A statistical information-flow model is described in which such activities would find a natural place. Discussion is confined mainly to the manner of concept-formation and concept-handling in such an automaton. It is suggested that the correlate of perception (as distinct from reception) is activity which organizes an adaptive internal response to signals from receptors, (as distinct from a "push button" type of response). This organizing activity amounts logically to an internal representation of the feature in the incoming signals to which it is adaptive, i.e., the feature which is thus "perceived". The adaptive response will normally consist not only of impulsive activity, but also of activity altering the "thresholds" of statistical links, which govern the probable sequences of activity in given circumstances. Thus information is represented in the model not merely by the current pattern of excitation but also by the configuration of thresholds governing its sequel.

A hierarchic structure is postulated wherein much of the organizing activity is concerned with modifying the probabilities of other activity. Abstract concepts and hypotheses are represented by sub-routines of organizing activity determined statistically by a corresponding sub-configuration of thresholds. These can in principle be evolved as a result of experience in a manner analogous to—or at least fruitfully comparable with—the process of learning and discovery; and it is not necessary for the designer to predetermine, nor possible for him to foresee, all the conceptual categories in terms of which the information-flux may be structured. Particular attention is directed to the conditions under which such a process could converge with sufficient rapidity.

Some of the symptoms of psychopathology find apparent correlates in possible modes of malfunction of such a system. But in this as in other respects it is not intended to press the resemblances. The intention is rather to stimulate critical comparisons, in order that the *differences* between the information-flow model and the real thing may continually lead to progressive refinement and enlarged understanding.

## SYMPOSIUM

### CROSS-NATIONAL RESEARCH IN SOCIAL PSYCHOLOGY

### PSYCHOLOGIE SOCIALE SUR LE PLAN NATIONAL

*Chairman — Président*

DANIEL KATZ (*Michigan*)

*Discussion*

ANGUS CAMPBELL (*Michigan*), BRADFORD B. HUDSON (*Houston*),  
OTTO KLINEBERG (*Paris*), GARDNER MURPHY (*Kansas*)

*Papers — Communications*

### CROSS-NATIONAL RESEARCH: THEORETICAL AND PRACTICAL CONSIDERATIONS

BY

HUBERT C. J. DUYKER (*Amsterdam*)

The first part of this paper will present a discussion of some general problems of cross-national research, the second part will give a brief outline of some of the specific features of a research project carried out by the Organization for Comparative Social Research.

#### I. CROSS-NATIONAL RESEARCH

##### A. *Necessity*

Empirical methods are essentially comparative. Differences in human behavior seem to a certain extent related to differences in nationality. Only comparative cross-national research can show where and to what extent correlations between behavior and nationality exist. Consequently, it is indispensable in assessing the generalizability of findings obtained in any one country.

##### B. *Specific Research Areas*

The nation is a prevalent form of social organization. Certain areas of research can only be explored by means of cross-national studies. For instance: the influences of various kinds of social structures, of educational systems, of specific national customs, of language. By using cross-national

comparative methods, social psychology can take advantage of the existence of a number of different, non-replicable, relatively stable social settings.

The study of international relations—in so far as it falls within the scope of social psychology—also requires a cross-national approach.

### *C. Some Major Difficulties in Cross-National Research*

1. *Comparability.* In order to be of scientific use, the data gathered in cross-national studies have to be comparable. In so far as the settings and the means by which the data are obtained are determined by the specific culture of a given nation, comparability is questionable. Studies in widely different countries may bring out contrasts clearly, exact comparisons are practically impossible. Studies in relatively similar countries make for easier comparability, but make necessary the use of rather refined measurement techniques to bring out such differences as do exist.

Comparability cannot be achieved by creating formally identical research settings. Comparison is possible between functionally equivalent situations. The criteria for functional equivalence are dependent on the general conceptual frame-work adopted.

2. *Communication.* Insofar as cross-national studies are genuinely co-operative studies carried out by social scientists in different countries, problems of communication tend to arise. Apart from the obvious—although not trivial—difficulty inherent in the existence of the various national languages, there is the even more serious problem of divergent training, background, experience, capacities and interests. In the absence of a generally recognized conceptual system, many psychologists use a terminology which, to say the least, is not easily understood by others.

## II. SPECIFIC FEATURES OF O.C.S.R. PROJECT

- A. Only nations in Western Europe participating. As homogeneous a group of nations as can be expected to be found anywhere, although the differences are considerable.
- B. Both survey and experiment used. Hypotheses tested by means of both methods.
- C. Hypotheses related to behavior of group members under conditions of threat.
- D. Special measures taken to guarantee comparability of data.
- E. Synchronization of operations in participating countries.
- F. Provisions for frequent contact between participating teams and for coordination of their activities.

CROSS-CULTURAL EXPERIMENTAL RESEARCH:  
METHODOLOGICAL PROBLEMS AND FACTUAL FINDINGS IN  
AN INTERNATIONAL STUDY IN GROUP BEHAVIOR

BY

STANLEY SCHACHTER (*Minnesota*)

I. As a basis for consideration of the methodological and interpretative problems involved in replicated, cross-cultural research, an experiment conducted by the Organization for Comparative Social Research in Holland, Sweden, France, Norway, Belgium, Germany and England is described.

II. The experiment:

A. Problem: Reactions to a deviate from a group norm are studied in the experimentally produced setting of a threatened group goal. Threat is defined as the possibility that a goal will not be achieved and it is assumed that the magnitude of threat is a function of the valence or desirability of the goal and the probability that a goal will not be achieved.

B. Design: The independent variables, goal valence and probability of reaching goal, were varied so as to produce four experimental conditions:

1. High valence, low probability of reaching goal.
2. High     "     , high     "     "     "     "
3. Low     "     , low     "     "     "     "
4. Low     "     , high     "     "     "     "

In the various countries there were an average of 8 groups in each condition.

C. Experimental setting: The experiment was conducted in the guise of the first meeting of a boy's aviation club. In the several conditions, valence was manipulated by establishing goals of varying desirability. Probability was manipulated by instructing some groups that they had a very good chance of reaching their goal and other groups that they had a very poor chance. In order to decide which groups would reach their goal, a competition was announced in which the groups building the best model airplanes would achieve their goals. The boys were presented with five possible models to build, and they were asked to decide which of these models they would like to build. Four of the models were extremely attractive, motor-driven planes, a fifth was an uninteresting glider model. One member of each group was a trained confederate who always chose the glider. The remaining members almost always chose one of the other

four planes. In each group, the subjects had a twenty-minute observed discussion of which plane to choose.

D. Measures of the dependent variable:

1. Rejection: Following the discussion, two sociometric-type measures were administered:

- a. A social preference scale.
- b. A role preference scale.

2. Influence exerted: Each discussion was systematically observed by two observers in order to provide a record of the influence process.

III. Methodological and interpretive problems of replicated research: The unique and central problem of replicated research is simply put in the question, "If there are differences in the results of the several experiments, how are they to be explained?" Such differences may be attributed to many factors:

A. Experimental artifacts, e.g. failure to properly manipulate the independent variables; poor translations of measuring instruments; etc.

B. Cultural differences in the several subject populations.

C. The original theory underlying the experiment may have been incorrect or only partially correct and an alternative formulation may be able to reconcile apparent divergent results.

Experimentally obtained differences attributed to each of these factors are presented.

IV. In order to be able to choose among these several interpretations of possible differences, it is necessary in any set of replications, to evolve measures which will allow evaluation of the comparability of the several experiments. In the present study, measures were devised to evaluate in each of the several countries:

A. The success of the manipulation of the independent variables.

B. The comparability of the verbal behavior of experimenter and confederates.

C. The extent to which observers in the several countries had common understanding of the categories in the observation schedules. What is international observer reliability?

V. The necessity for standardization: Though measures such as those in IV may allow us to determine if the difference between experiments is attributable to experimental artifacts, such a finding is of trivial import.

It is desirable to eliminate such factors and to maximally standardize the several experiments. Procedures employed to standardize experimenter, confederates, observers and measures are described.

VI. Conceptual equivalence and standardization: Though it may be possible to create experiments which in execution are virtual images of one another, this alone is no guarantee that the several experiments produce conceptually equivalent situations. An effective manipulation or experimental setting in one nation may be completely unsuccessful in another. Illustrations from present research are presented.

#### A COMPARATIVE STUDY OF TEACHERS' ATTITUDES TO INTERNATIONAL PROBLEMS AND POLICIES: PRELIMINARY REVIEW OF RELATIONSHIPS IN INTERVIEW DATA FROM SEVEN WESTERN EUROPEAN COUNTRIES

BY

VILHELM AUBERT, BURTON R. FISHER, STEIN ROKKAN (*Oslo*)<sup>1</sup>

During 1952 seven Western European teams of social scientists entered into cooperative arrangements, agreeing to conduct a set of comparative pilot inquiries into attitudes to international problems and policies. Interaction via conferences and mail led to exploratory interviews and agreement on design and on basic field instruments for data collection. Intensive interviews with samples of 300 primary and 100 secondary school teachers in Belgium, England, France, the Netherlands, Norway, Sweden and Western Germany were carried out early in 1953. To make for comparability, instructions and instruments prepared by national teams were checked centrally before use, and methodological and technical problems were regularly discussed with the research co-ordinator, Dr. Eugene Jacobson, on his visits to all of the teams.

The interview questions, including open-ended items as well as lists of statements for checking by respondents, covered the teacher's professional problems, views on principles of behavior and discipline, satisfaction with his job and perception of his social position, as well as reactions to international issues, defense policies and domestic politics. National teams coded their own interviews on the basis of common-core codes set up centrally. The analysis of the data (entered on IBM cards) directly concerned with attitudes to international problems and policies is in progress; the present account is only a very preliminary report.

<sup>1</sup> This paper was read by Sverre Lysgaard.

The original design of the attitude studies was based on a model relating increased threat to the values of a group to increased pressures toward conformity. Interviews explored four aspects of respondents' perception of threats in the current international situation: (1) *salience* of worries about the international situation; (2) *probability* of different world developments occurring; (3) *valence* ordering of alternative developments; (4) *principal sources* of dangerous developments. Each of these perceptions have been related in analysis to each other and to measures of (1) the respondent's *opinions* on his *government*, its domestic policies, defense efforts and general management of foreign affairs; (2) his *perceptions* of *national unity* and *disunity*; (3) his optimism-pessimism on the possibilities of citizens *influencing events*; (4) his opinions about *dissent* and about *deviants from national policy*.

Findings so far provide the basis for a distinction between two kinds of emphasis in the teacher respondents' threat perceptions: (1) enemy-oriented emphasis, focussed on a potential aggressor; (2) war-oriented emphasis, focussed on the danger of war as such. Enemy-oriented respondents are significantly more often in favour of increased defense efforts, more often express confidence in their governments' foreign policy (N.B.—except in Sweden), are more often conservative in politics, more often express satisfaction with their government's domestic policies (in all countries governed by conservatives) and tend to see wider national consensus on matters of defense than the war-oriented (in all countries except Sweden). Relationships between threat orientation and opinions about dissent and deviants are less clear; significant associations are found in France, the Netherlands, Norway and Sweden.

## OUTLOOK OF YOUTH IN TEN COUNTRIES

BY

GORDON W. ALLPORT (*Harvard*)

The study is based on an inquiry directed to 1,900 arts and sciences students in institutions of higher education (most between 17 and 25 years of age) from the following countries: United States—Harvard, Radcliffe, Miami University (Ohio); New Zealand—Victoria University College; South Africa—University of Natal, University of Stellenbosch; Egypt—Higher Training College, Woman Teachers' Institute; Mexico—University of Mexico; France—Faculté des Lettres, Bordeaux; Italy—Catholic University of the Sacred Heart; Germany—Technische Hochschule,



Braunschweig; Israel—Hebrew University; Japan—Tokyo University. The sample in most countries totalled approximately 200 students, evenly divided between the two sexes.

The study took the form of asking each student to write an autobiography, "From Now to 2,000 A.D." and also to answer subsequently a series of fifty questions. The inquiry forms were presented in the language of each group and the replies were then translated. They will be fully summarized at a later date by Mr. James Gillespie in the Department of Social Relations at Harvard University.

The questions asked were designed to bring out each student's present physical, economic, occupational, social and educational status and the way he regards the future. Questions concerning the future included: what he would most like to know about the future up to 2,000 A.D.; the occupation expected and most desired; when he expects and would most like to retire; attitudes toward careers for women, toward marriage and families, toward place of residence, travel; persons in whom he would confide; personal friendships; sources of and periods of greatest satisfaction; expectations concerning living standard; ways of getting ahead; principal fears; sexual standards; degree of nationalism and internationalism; degree of dependence upon their own initiative or upon others; expectations regarding war, peace, and forms of international co-operation; race attitudes and attitudes toward democracy and religion.

Among the similarities among nations we note that all are interested in the possibilities of interplanetary travel; the majority of youth feel the need of religion (excepting Israeli, French, and Japanese). All favor closer and more permissive parent-child relationships. There is widespread desire for travel, and favorableness toward world unity, with U.S. and Japan most favorable; Afrikaners and Egyptians most nationalistic.

Differences in values are also clear. U.S. youth do not include the social and political context of their lives in their outlook on the future as much as do youth of other nations. U.S. youth seem more "privatistic" and plan for a rich, full life without clearly expressed social concern. A belt of special pessimism of outlook seems to rim the Mediterraneans; the farther from this area the more evidence of optimism and voluntarism in outlook. Many other revealing differences indicate that this method of co-operative international research will enlarge the vision of social psychology as a worldwide science.



## SYMPOSIUM

### PROBLEM SOLVING BEHAVIOR

#### LE COMPORTEMENT DANS LES SITUATIONS PROBLÉMATIQUES

*Chairman — Président*

BENTON J. UNDERWOOD (*Northwestern*)

*Discussion*

ROBERT M. GAGNÉ (*San Antonio*), ERNEST R. HILGARD (*Stanford*)

*Papers — Communications*

#### THE PROBLEM OF PROBLEM SOLVING

BY

GEORGE HUMPHREY (*Oxford*)

When considering the problem of problem solving it is important to take a biological view of what is happening; that is to bear in mind that the activity described is that of an organic system working in an environment. It is assumed that changes from within or without the system give rise to systematic or organic processes, of a kind ordinarily called behaviour: following Boring, conscious experience may be included under this rubric. Since, almost by definition, the organism acts as a unity, and since the changes (stimuli, Verworn) which affect the organism are diverse, the living system must comprise mechanisms for the integration or unification of response. These have been studied by workers of many different disciplines, from protozoologists down. In the course of organic evolution and following the process laid down by Murphy and Coghill, differentiation has taken place within the organic system, with the formation of partial or sub-systems. These improve the chance of systemic survival and at the same time take their place in the integrative mechanism of the total system. Instances are the receptors ("analysers" of Pavlov), the central nervous system, and the blood. When there is reaction, organic changes are set up

which, by the activity of special mechanisms, are directed along lines laid down either by the organism's original constitution (organic survival, sex, as examples) or by others which have been acquired, such as perhaps drug-hunger. In certain cases, however, delay may occur, as with what are called problem situations. Here it seems that the ordinary process of reaction is magnified; there being little evidence that problem solving differs from any other response, except that it is longer drawn out and therefore its working may more easily be examined.

Out of the many problems of problem solving the following are (of necessity briefly) considered. (1) Trial and error, or, according to Krechewsky, hypotheses; (2) the directiveness of activity; (3) integration, of originally diverse activities or environmental features; (4) the relation of the activity of the partial organic systems to that of the whole, (e.g. perception, images, language); (5) stereotyped activity as related to (4); (6) concept formation.

## VERBAL FACTORS IN PROBLEM SOLVING BEHAVIOR

BY

HOWARD H. KENDLER (*New York University*)

The importance of verbal factors in reasoning has long been recognized by all psychologists who have endeavored to understand problem solving behavior. This paper will deal with an attempt to extend some aspects of S-R learning theory to problem solving behavior by considering implicit verbalizations as response produced stimuli. This orientation essentially extends the concept of a stimulus from a directly observable concept in the conditioning experiment to an inferred theoretical concept in the human problem solving situation. Although there is an apparent similarity between the concepts of response produced stimulus and the psychological field of the phenomenologically oriented psychologist, a basic difference does exist between these two theoretical constructs. This difference stems from the methodological commitment of behaviorism to define theoretical concepts in terms of experimentally manipulatable variables.

This behavioristic approach is demonstrated by reference to a series of studies generated by the hypothesis that concept formation behavior in a card sorting situation, on any one trial involves a sequence of two S-R associations. According to this formulation, the stimulus component of the first association would represent the test cards, while the response would refer to implicit verbal or symbolic responses made to them. The stimulus

of the second association would represent the cue produced by the preceding implicit response, while the response would be the overt card sorting behavior. The specific aspect of this mediational hypothesis which was tested was that the presence of the "appropriate" symbolic cues (the stimulus of the second association) even though they might be connected to the "wrong" sorting response would facilitate concept formation.

This hypothesis was applied to a situation involving the learning of successive concepts in a multiple solution card sorting problem patterned after the Wisconsin Card Sorting Test. For one group (reversal shift) the second concept to be learned was the reverse of the first concept, i.e., the cues were reversed so that the *S* was required to sort the cards in a fashion opposite to that demanded initially. For the other group (nonreversal shift) the second concept was unrelated to the first concept in the sense that the basis of the correct sorting was shifted from one stimulus dimension to an entirely different one. Because the reversal shift groups would have the appropriate symbolic cues present at the time of the shift it would be expected that a reversal shift would occur at a more rapid rate than a nonreversal shift.

The results indicated that a reversal shift did occur more rapidly than a non-reversal shift. This superiority of the reversal shift occurred when partial reinforcement effects were eliminated from the non-reversal shift. The findings also indicated that a reversal shift produced positive transfer effects while a non-reversal shift resulted in negative transfer effects.

The relationship between these results and problems associated with extension of S-R conditioning theory to human reasoning behavior is discussed.

## A VARIATIONAL APPROACH TO THE ROLE OF SET IN PROBLEM SOLVING

BY

A. S. LUCHINS (*McGill*)

The basic design consists of a series of arithmetical problems in each of which a specified volume of (hypothetical) fluid is to be obtained through manipulation of (hypothetical) jars. The first five problems, the set-inducing tasks, are all solvable by one method. There follow two kinds of problems: *critical tasks*, solvable by the oft-repeated (set) method as well as by more direct procedures; and an *extinction task*, solvable only

by a more direct method. Subjects who received only the test tasks almost invariably solved them by direct methods while those who received all the problems, often failed to use the direct methods in some test tasks. This may be taken as evidence of the effect of an *Einstellung* or set.

For almost two decades a variational approach has been applied to this basic design, yielding hundreds of experimental variations and involving thousands of subjects. A variational approach is one which studies a specific phenomenon under a variety of conditions, including external stimulus conditions and internal states of the organism. Experimentation was systematized through an extremum principle. Specifically, we sought to maximize or minimize the strength of the *Einstellung* effect.

Most successful in tending to maximize the *Einstellung* effect were the following variations:

1. *Increasing the number of set-inducing problems.* The results are pertinent to the relationship between (a) repetition of response and repetition of stimulus and (b) frequency of response and habit-strength.
2. *Creation of a "stress-atmosphere".* This was achieved by presenting the experiment as a speed test and by arousing anxiety and tension.
3. *Instructing subjects to generalize a rule of solution.*
4. *Presenting the tasks as in an "isolated drill" lesson.*
5. *Holding constant in the set-inducing problems both the method and the volume (goal) to be obtained.* Incomplete test problems, in which some of the jars or the goals were specified by the subject, allowed comparisons of the relative strength of method-set and goal-set.

Most successful in tending to minimize the *Einstellung* effect were the following:

1. *Increasing the number of extinction problems.* The results have bearing on the issue of whether a habit is extinguished through disuse or through learning of a new, contradictory habit.
2. *Interspersing critical tasks and extinction tasks among the set-inducing problems.*
3. *Offering clarification concerning the nature of sets and their possible deleterious effects.*
4. *Increasing the number of jars in each problem.* The subject could not simply impose the oft-repeated method on a task but had first to examine the task in order to discard superfluous jars.
5. *Reversing the complexity of the set and non-set methods.* When the set-inducing problems were solvable by a direct procedure, less *Einstellung* effect was obtained in the usual test tasks. This raises the question of the roles played by (a) the relative complexity of the set and

non-set procedures and (b) the means used to assess the influence and strength of set.

Implications were drawn for what can be done by educators and teachers to enhance beneficial influences of habits while minimizing the blinding effects of habits.

The variational approach was offered not as a substitute for theoretically oriented research but as another rather fruitful approach to research.

## PATTERNS OF INDUCTIVE THINKING

BY

B. INHELDER (*Geneva*)

How does the child—or the adolescent—become able to organize an experiment? Experimentation is more than mere manipulation of objects. It involves a whole set of processes, a truly inductive method. Induction is an experimental method of examining reality as to its logical structure.

We have resorted to mechanical devices and materials which allow the independent discovery of laws and causes. In order that the material may be manipulated by children of different stages of development, the material must permit at the same time simple handling or complex discoveries. Seventeen-hundred children and adolescents from 5 to 16 years have been examined individually. To give a true picture of the formation of experimental behavior, we shall proceed through cross-section and longitudinal analyses.

### *Cross-section Analysis:*

In every experiment one can distinguish the goals of action, the experimental organization, the interpretation of results and the method of checking. These four factors are not successive but always intricately joined.

### *Longitudinal Analysis:*

Although the evolution of experimental behavior is continuous, we notice periods of profound changes which mark the appearance of a new stage of development. Three important stages may be distinguished:

(1) The first stage of "play techniques" extends from 5 to 7 years. The goals of action are the pleasure of being the cause of something. The organization consists in a kind of indiscriminate intervention into the physical world. The interpretation of results consists in finding again in the physical world what the young child wants to find there. Checking

must be called pseudo-checking because the child remains more or less impervious to the results.

(2) The second stage, characterized by the formation of "concrete techniques", extends from the age of 7 to 11. The goals of activity are oriented towards the discovery of practical rules. The organization at this level becomes a discriminating analysis of the experience. The interpretation of the experiment gains in objectivity and requires a series of concrete operations, such as to establish connections, classifications, successions; to count and to measure. Checking reflects the degree of the generalization of the laws discovered.

(3) The third stage of "scientific techniques" begins in pre-adolescents and finds its completion at about 14 to 15 years. The goals consist in trying to find the truth and not simply in the controlling of the events. The experimental organization reaches its true value only at the adolescent level. In fact only the adolescent is able to draw up a plan of execution and to make an estimate of possible factors. The interpretation of results consists in translating the concrete facts into abstract notions. The adolescent tries, for example, to use systems of reference of a geometric order to translate physical facts. The methods of verification are not limited to indiscriminate interpretation of the facts but lead to a complete separation and combination of all the interdependent factors.

The inductive method is thus a final form of intelligence, integrating into a structured whole all the preceding stages of adaptive behavior. The inductive method involves a new mental structure, following precise laws such as the laws of "group" or "lattice", which according to Cybernetics can be both of a mathematical and physiological nature.

## PROBLEM SOLVING BY GROUPS

BY

DONALD W. TAYLOR (*Stanford*)

Assume that of a given population of subjects,  $p$  will solve and  $q$  will fail a particular problem. The probability of drawing at random from that population a single subject that will fail the problem is  $q$ ; the probability of drawing  $k$  subjects, all of whom will fail is  $q^k$ . If working in a group has no effect on individual performance, then the probability that a group of  $k$  size will solve a problem is given by  $1 - q^k$ ; the tenable assumption is made that the group solves the problem if any member solves it. This simple model is applicable to problems solved in one or in several steps;

it does not assume homogeneity of subjects. (It should be mentioned here that Irving Lorge and Herbert Solomon of Teachers College, Columbia University, have independently formulated and employed this same model.)

The model represents a kind of null hypothesis—i.e., the performance of the group to be expected on a probability basis if there is no effect, positive or negative, of working in the group. By comparing the predicted with the actual proportion of groups solving a problem, one can determine whether working as a member of a group significantly affects individual performance. Dr. Lincoln Moses has worked out a likelihood ratio test appropriate for testing the difference between the predicted  $p$  and the obtained  $p$  for groups. Unfortunately the results from more than one problem can not be combined. Because of this limitation, an alternative method of comparing actual group performance with that predicted from individual performance has been devised.

This method has been used in an experiment recently carried out in cooperation with W. L. Faust and R. J. Rhine. Subjects were randomly assigned to work either as individuals or as a member of a group of four. After completion of the experiment, the 64 who had worked alone were randomly divided into 16 nominal groups of four each. The performance of each nominal group on each problem was scored as though the nominal group of four had actually worked together—i.e., if any one of the four solved a problem, the nominal group was considered to have solved it. The difference between the performance of the 16 nominal groups and the 17 real groups was not statistically significant. This experimental design provides a test of the same kind of null hypothesis as does the model described above, but has the advantage of combining the results from several problems.

## SYMPOSIUM

### RECENT DEVELOPMENTS IN COLOR

### NOUVELLES DÉCOUVERTES AU SUJET DE LA COULEUR

*Chairman — Président*

WALTER R. MILES (*Yale*)

*Discussion*

Sir FREDERIC C. BARTLETT (*Cambridge*), LOUISE L. SLOAN (*Johns Hopkins*)

*Papers — Communications*

### LE PROBLÈME DU RÔLE DES PROCESSUS RÉCEPTEURS, TRANSMETTEURS ET INTERPRÉTATEURS DANS LA VISION CHROMATIQUE

PAR

HENRI PIÉRON (*Paris*)

L'extrême complexité de la vision des couleurs, entraînant d'innombrables désaccords, est due à l'intervention : 1) d'une combinaison initiale de processus récepteurs comportant plusieurs réactions photochimiques avec des interactions nerveuses préganglionnaires ; 2) de l'envoi simultané de messages optiques spécifiques passant par un important relai ; 3) d'une réception des messages dans la zone de projection corticale ; 4) d'une interprétation perceptive des messages dominée par des influences de conditionnement.

Il est donc nécessaire de faire la part des processus relevant des étapes successives. Or, des théories cherchent à rendre compte de tout l'ensemble des faits d'un unique point de vue (celui des processus photochimiques avec Selig Hecht), et l'erreur fondamentale de Hering était de confondre les processus perceptifs de leucie (*lightness*) et chromoleucie avec des sensations primaires.

Quelques données peuvent déjà être dégagées.

Dans l'*adaptation chromatique*, le processus principal est d'origine oculaire : perte de saturation liée aux interactions nerveuses d'équilibre



instable (inhibitions réciproques des trois systèmes fondamentaux) ; une sensibilité, réduite de 1 à 0,27 en 30 s. d'éclairement rouge, remonte à 0,35 en 40 s. d'obscurité, mais à 0,44 en lumière blanche isophane et à 0,88 en lumière verte de même durée. D'autre part des processus d'adaptation perceptive sont manifestes (après port prolongé des lunettes bicolores d'Ivo Köhler).

Dans le *contraste*, le rôle des phénomènes perceptifs est incontestable, mais il existe un *contraste préperceptif*, comme dans l'expérience classique de Hering (croix grises vues, l'une sur fond rouge par un œil, la seconde sur fond bleu par l'autre et perçues différemment verte et jaune alors que le fond est uniformément pourpré en vision binoculaire) et dans celles de Sherrington (reprises au point de vue chromatique par mon élève Lucile Bailey Mahieu en 1923), où un effet de contraste non perceptible accroît une fréquence critique de fusion sur disques tournants.

Pour les *tonalités chromatiques*, le « bleu » des bâtonnets résulte d'un phénomène de conditionnement perceptif, mais le dichromatisme de la périphérie rétinienne n'est toujours pas élucidé (absence probable de récepteurs corticaux, avec maintien des récepteurs rétiniens). L'absence du jaune que j'ai pu constater avec Galifret dans la région tritanope centro-fovéale paraît liée à l'absence des récepteurs périphériques du bleu comme dans la tritanopie congénitale (à bande grise spectrale dans le jaune), l'interinhibition totale des deux systèmes récepteurs restants devant entraîner l'achromatisme caractéristique de la lumière blanche, tout comme l'interinhibition normale des trois systèmes.

L'origine de l'inégalité des *retards propres* des trois fondamentales que j'ai étudiée depuis 1923 reste encore en suspens : vitesses différentes de processus préganglionnaires (d'après Monnier, 1949, sur l'onde *b* de l'erg, et Donner, 1950, sur les réponses « on » et « off » du chat), ou de propagation d'influx (d'après Chang, 1950), ou de processus corticaux ?

Une étape importante dans le progrès de nos connaissances serait assurée par une analyse des fibres optiques qui réussirait à différencier les vectrices des messages fondamentaux.

## THE ROLE OF LIGHT AND COLOR IN PSYCHOBIOLOGY

BY

AUSTIN H. RIESEN (*Chicago*)

As a fundamental source of energy for biological functions, light exercises its effects by first initiating photochemical events. The behavior of the organism may then be quite directly brought into action through

the nervous system, or it may be influenced subsequent to complex physiological changes. Some of these processes occur only in specific animal forms. Others occur throughout wide ranges of the zoological scale.

The behavioral effects of light may be grouped into at least three major functional categories which apply generally to all types of organisms, including man. First, there are those effects which modify motivational states, via hormone systems and/or by way of direct alteration of effector tonus and excitability. Diurnal and seasonal rhythms are ubiquitous examples of oscillating changes in thresholds which depend on hormonal and neural rhythms. Migratory and reproductive behaviors are special cases.

Orienting responses to light, especially blue light, are exhibited by plants and by animals from amoeba to man. Tropisms, taxes, kineses, reflexes, and learned orientations may all be initiated by the various gradients of photic excitation. Polarization is a factor in some arthropods, i.e., forms having compound eyes. Reflex head and eye movements are rather generally homologous throughout much of the vertebrate series from fish to man. These assure a rapid change in orientation which facilitates the most efficient use of the receptor.

A third class of reactions to light may be broadly defined as those responses which constitute identifying (or recognition) behavior. Here a discrete configuration of light or color leads, singly or in combination, to approach, manipulation, ingestion, avoidance, autonomic reactions or other discrimination responses. The human identification response is almost universally one that attaches itself to a particular stimulus configuration through learning, and the response itself is often a product of earlier learning. Insects, fish and birds, on the other hand, show much non-associative identifying behavior. Color comes into great prominence in some examples, as in the social behavior of fighting fish and the color preferences of newly hatched chicks.

The pervasive role of light and color in human behavior can scarcely be over-emphasized. Recent evidence suggests the possibility that fundamental growth processes may be hastened or retarded. Precise visually mediated adjustments in space develop from an innate substrate through learning processes that require hours of daily exploratory behavior during the early years of life. In man, responses to color are a highly individual matter. They may depend predominantly on specific environmental factors. Research may yet show conclusively, however, that affective states produced by "warm" colors and by gloomy skies are more than red-integrative responses dependent upon earlier environmental contiguities.

METHODOLOGICAL REQUIREMENTS IN THE STUDY OF  
NORMAL AND DEFECTIVE COLOR VISION

BY

DEAN FARNSWORTH (*New London*)

A survey of color vision research in the past half century and of programs now underway suggests that the following categories of methodological tools are most necessary:

1. complete specifications of stimuli in terms of primary and secondary physical standards;
2. explicit control of physical, psychological and physiological variables affecting vision; and
3. definition of the experimental population—selection and classification.

In particular there appears the need to develop a better defined psychophysical discipline, to re-examine certain classical assumptions, to adhere to operational definitions, and to extend quantification of certain qualitative phenomena.

Data defining the present status of color vision are presented in relation to the above requirements.

## COLOR AND THE RETINA

BY

LORRIN A. RIGGS (*Brown*)

There is still no direct evidence on the retinal basis for color vision. It is assumed that there are specialized receptors differentially sensitive to lights of various wavelengths, and that these receptors send impulses along nerve pathways having characteristic spatial or temporal patterns of action. No histological or physiological differences have been found as yet among the cone receptor cells believed to be responsible for color vision.

Much indirect information, however, has recently been obtained. Of major interest is the work of Granit and others in which microelectrodes have been used to record the activity of neural elements in a variety of animals. The responses so recorded are initiated by unknown combinations of rod or cone receptor cells. Nevertheless, by selective adaptation and polarization much has been learned of the differential sensitivity of retinal elements for various wavelengths of light.

A still more indirect measure of retinal activity is in the corneoretinal potential or electroretinogram. This method has the advantage that it can be used in the human eye, thus making possible direct comparisons between vision and retinal action. Specific wavelength effects in the human electroretinogram have recently been revealed in the studies of Adrian, Motokawa, Armington and others. These studies, while based on the mass activity of photopic and scotopic mechanisms of the retina, have already revealed interesting correlations with visual data on state of adaptation, retinal area, and color defects.

An entirely different approach is that of Motokawa and others who use electrical stimulation of the human eye to test specific wavelength effects. The sensitivity of the eye to electrical stimulation appears to be enhanced by flashes of light. The amount and time course of such enhancement are related to the intensity and wavelength characteristics of the flash.

Of fundamental importance are the new advances by Wald and others in the identification and synthesis of visual pigments. There is now an impressive array of such pigments having widely different spectral sensitivities. There is as yet no evidence as to their function in color vision, but certain parallels already suggest themselves between the spectral characteristics of the pigments and visual data from other sources.

We must conclude that while the exact mechanism of color vision is still largely a matter of speculation, there is a rapidly growing body of knowledge about the color-differentiating responses of the retina. This knowledge comes from a variety of new methods of experimentation and provides a refreshing addition to the mass of psychophysical data on color vision. We may reasonably hope to see a better understanding of this field within the next few years.

## THE EXPRESSIVENESS OF COLOR

BY

RALPH M. EVANS (*Rochester*)

Using color photography to investigate the problems and to illustrate the points, an attempt is made to find the basic factors in the use of color in all situations. The various kinds of color are first reviewed. This is followed by a study of why color is used and the various ways in which it may affect people. Emotional, associative and expressive elements are considered, leading to the conclusion that deliberate expression is the most

important case. The rules of color harmony are reconsidered from this standpoint.

Harmony is one feeling that can be expressed by color but the rules, basically, refer to the form of the expression rather than the thought. The expressive elements of color are shown not to be simply hue, saturation, and lightness but to include all attributes and all modes of appearance of color. In these terms it is possible to generalize the concept of color schemes and at least make a start toward a true understanding of the ways in which the power of color may be applied successfully.

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# INDEX OF AUTHORS NAMES — INDEX DES NOMS DES AUTEURS

	Pages		Pages
Allport, G. W. . . . .	211	Jasper, H. H. . . . .	162
Asratyan, E. A. . . . .	135	Kendler, H. H. . . . .	214
Aubert, V. . . . .	210	Köhler, I. . . . .	176
Baerends, G. P. . . . .	93	Kubie, L. S. . . . .	123
Benton, A. L. . . . .	165	Lacey, J. I. . . . .	137
Biesheuvel, S. . . . .	126	Lagache, D. . . . .	145
Bott, E. A. . . . .	18	Leeper, R. . . . .	110
Bracken, H. von . . . . .	195	Lehrman, D. S. . . . .	96
Brady, J. V. . . . .	148	Lenep, D. J. van . . . . .	101
Brunswik, E. . . . .	108	Léontiev, A. . . . .	132
Buytendijk, F. J. J. . . . .	198	Le Senne, R. . . . .	198
Cook, S. W. . . . .	190	Lewis, D. . . . .	181
Coombs, C. H. . . . .	154	Likert, R. . . . .	170
Delgado, J. M. R. . . . .	163	Lippitt, R. . . . .	193
Duyker, H. C. J. . . . .	206	Luchins, A. S. . . . .	215
El-Koussy, A. H. . . . .	130	Lysgaard, S. . . . .	189
Evans, R. M. . . . .	224	MacKay, D. M. . . . .	204
Eysenck, H. . . . .	140	McClelland, D. C. . . . .	100
Farnsworth, D. . . . .	223	McGill, W. J. . . . .	203
Fisher, B. R. . . . .	210	McQuitty, L. L. . . . .	103
Frenkel-Brunswik, E. . . . .	98	Michotte, A. van den Berck . . . . .	70
Gibson, J. J. . . . .	178	Miller, D. R. . . . .	101
Graham, C. H. . . . .	114	Miller, N. E. . . . .	151
Granit, R. . . . .	117	Murphy, G. . . . .	111
Gunter, R. . . . .	115	Newman, E. B. . . . .	202
Guttman, L. . . . .	155	Nuttin, J. . . . .	158
Harlow, H. F. . . . .	152	Penfield, W. . . . .	47
Hartline, H. K. . . . .	118	Petrie, A. . . . .	167
Heider, F. . . . .	159	Piaget, J. . . . .	41
Hemphill, J. K. . . . .	174	Pichot, J. J. . . . .	166
Himmelweit, H. T. . . . .	143	Piéron, H. . . . .	220
Höhn, E. . . . .	143	Ponzo, M. . . . .	182
Hsia, Y. . . . .	114	Postman, L. . . . .	179
Humphrey, G. . . . .	213	Radhakrishna Rao, C. . . . .	104
Iersel, J. J. A. van . . . . .	95	Riesen, A. H. . . . .	221
Inhelder, B. . . . .	217	Riggs, L. A. . . . .	223
Israel, J. . . . .	142	Rokkan, S. . . . .	210
James, H. E. O. . . . .	192	Rosvold, H. E. . . . .	163
Jarl, V. C. . . . .	128	Schachter, S. . . . .	208

	Pages		Pages
Selltiz, C. . . . .	190	Tucker, L. R. . . . .	105
Smith, K. U. . . . .	184	Wallach, H. . . . .	180
Sokolov, E. N. . . . .	134	Watson, J. . . . .	193
Solomon, R. L. . . . .	150	Welford, A. T. . . . .	185
Stellar, E. . . . .	147	Wellek, A. . . . .	156
Stevens, S. S. . . . .	121	Westerlund, G. . . . .	171
Taylor, D. W. . . . .	218	Wilson, A. T. M. . . . .	172
Téplov, B. M. . . . .	138	Wrigley, C. . . . .	106
Thomae, H. . . . .	200	Yela, M. . . . .	129
Tolman, E. C. . . . .	31	Zaporozhets, A. B. . . . .	187
Toman, W. . . . .	124	Zazzo, R. . . . .	197
Tomkins, S. S. . . . .	160	Zilboorg, G. . . . .	122

# A DOUBLE-DIRECTED ANALYSIS OF PREFERENCE MOTIVATIONS AND OTHER PAL-DESCRIPTION STATEMENTS: STUDIES IN SOCIO-PERCEPTUAL SELECTIVITY

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The present paper is designed to give a methodological discussion of the fitness of pal-preference motivations and similar pal-description statements for psychological use. A two-way analysis of such statements is illustrated: (a) an object-directed analysis in terms of perceptions of pals as object-determined; and (b) a subject-directed analysis in terms of perceptions of pals as subject-determined.

## *The Interpretation of Choice-Motivations in Sociometric Preference Analysis*

Sociometric methods of analyzing interpersonal preferences are based on the "significance of choice in human behavior" (14). The subject is required to choose co-workers, co-players, etc., i. e. to express his desire to interact with certain individuals, as well as his desire to avoid interaction with certain others – always with reference to specific situations (specific choice aspects).

In his basic text from 1934 (cf 18, p. 328 seq.) Moreno wrote that the analysis of the spontaneous choices ought to be followed by an analysis of the choice-motivations. Of course, most people are unable to explain the origin of their attitudes in a satisfactory way; in fact, the choice is felt by Moreno to be much more significant than the motivation. Thus in the large body of sociometric investigations published during the last twenty years, these difficulties seem to have led most sociometrists to avoid the problem altogether: i. e. the choices are often analyzed as behavioral facts without any attempt at investigating statements of choice-motivations. However, it seems that these difficulties should be further analyzed.

Due to the complexity of both motivation and perception, it is obvious that the *Phäno-motiv* – the motive within the subject's "field of awareness" – often does not coincide with the *Geno-motiv*: the "real" motive (24, p. 563-570).<sup>1</sup> "Conscious" and "unconscious" motives may fuse and "dis-

1. But neither is it certain that what seems to be a "cause" from the point of view of the experimenter is a functioning motive; it may as well be only a *Phäno-motiv* on another level of description (cf 17, p. 79).

charge" in the same choice. In the subject's perceptions of his motives, we probably have to consider not only perceptual end-stages, but also liminal forms and subceptions – perceptions outside clear awareness – which may affect already established percepts, although not realized by the subject as separate facts (cf 23). On the other hand, the unreal *Phäno-motiv* may develop into a *Geno-motiv* (a "rationalizing" may become "fixated") and partly direct future behavior. Finally – and this is a problem on another level – the verbal statement may not only be an "unconscious lie", i. e. represent a self-deception, but also be a "conscious lie", i. e. be an attempt to deceive the experimenter. The present writer certainly does not pretend to deal with all these complexities in detail, and still less to disentangle them. However, an attempt will be made to show that some ways of dealing with preference motivations and equivalent pal-description statements may be fruitful, *although* these statements cannot be considered to have immediate "face validity". Psychology ought *also* – and some would perhaps say *in the first place* – to take account of "subjective facts", subject-determined representations *and* misrepresentations of "objective facts". We may express this important point in another way by using terms from Kecskemeti's semantic analysis (cf 15, p. 32): although we cannot always rely on a "text interpretation" of the "communicative meaning", we may perhaps make some use of a "context interpretation" of the "situational meaning". Regardless of whether the communications are "true" or "false", they are behavioral units of the communicator, and some interesting relation between these and other units of behavior may be found.

Another question, however, is whether we are allowed to discuss such preference motivations without reference to the special choice-relations which they were primarily intended to illustrate. If  $x$  chooses  $y$ , and explains his attitude with the motivation  $M_x$ , and if  $y$  chooses  $x$ , and gives the motivation  $M_y$ , we have not only the preferential relation  $xPPy$  but also one kind of motivational relation<sup>2</sup>, which may be symbolized roughly thus:  $xM_xM_yy$ . If we only discuss  $xM_xy$  or only  $yM_yx$ , we have already withdrawn one step from "reality", and when, finally, we discuss the motivations  $M_x$  and  $M_y$  alone, we have retired still further. When we proceed as in the last case, we must always remember that we are dealing with fictitious purifications (cf 4), but in cases where we have the possibility of achieving new insight by such a procedure, we cannot exclude it *a priori* (cf. 20).

Some investigations have also been made in order to classify the ver-

2.  $xPPy$  is used as a condensation for a mutual positive attitude between  $x$  and  $y$  ( $x$  chooses  $y$ , and  $y$  chooses  $x$ ; for different types of sociometric "formalizations", cf 2,3).

balized preference motivations (cf e. g. 6,26). In a sociometric investigation of Swedish school children the present author made an analysis of their motivations in connection with choices of co-workers (to be reported at length elsewhere; for details of the sociometric questionnaire, cf. 3, p. 75-76).

*A Pal-Description Device on the Basis of Preference Motivations*

From the discussion above about the "objectivity" of the preference motivation, we may conclude that the preference motivations may be treated as a kind of double-directed pal-description: on the one hand they aim to describe the behavior of other persons, on the other hand they are also so intimately associated with the communicator that they may describe him too. In some respects the one way of analyzing the motivations may be the most promising, in other respects the other way is the only possible. In order to be able to clarify some aspects of these problems, it may be of some use not to start with the spontaneous choice motivations, which are sometimes too complex, but instead to use similar statements in a special pal-description device. Moreover such a device may be used in its own right for certain purposes, and may therefore be of some general interest.

On the basis of the analysis of choice motivations we may construct a simple pal-description device by choosing characteristic motivations with reference to different positive and negative "traits". When the present author did so in an investigation of Swedish school children, an attempt was made to retain the expressions of the children as far as possible, but, of course they are not always capable of accurate translation. The finished form consisted of 36 items which were intentionally arranged unsystematically. However, the construction of the form will be clear from the following arrangement (the figure after each item shows its place in the form as it was given to the pupils):

I. Social "positivity" vs. social "negativity".

1. Is nice to talk to. You could chat with him (her) about everything. 31.  
Is difficult to talk to. 16.
2. Willingly lends things to those who need something, and gladly shares with others if he (she) has something to offer. 19.  
Is rather stingy: is hardly ever willing to lend anything to others or offer anything. 4.
3. Is very helpful. Helps at once when somebody needs help. 13.  
Is not helpful. Helps others only reluctantly. 28.
4. Is reliable and trustworthy. Never tells tales. 7.  
Is unreliable and untrustworthy. Often tells tales. You cannot confide in him (her). 22.

5. Is never quarrelsome. Does not quarrel or fight. 25.  
Is rather quarrelsome. Quarrels or fights rather often. 10.
6. Never goes to pieces or looks sullen. Is always cheerful and lively. 1.  
Easily goes to pieces or looks sullen. 34.

II. *Extrovert social activity vs. introvert social passivity.*

1. Loves to talk himself when there are lots of people around. 17.  
Hates to talk himself when there are lots of people around. 2.
2. Gets acquainted easily with newcomers. Is not at all shy. 35.  
Has difficulties in making new acquaintances. Is shy and timid. 20.
3. Prefers to play and work together with others. 23.  
Prefers to play and work alone. 8.
4. Often starts new games. Likes to organize and decide. 11.  
Seldom starts new games himself. Is hardly ever willing to organize and decide. 26.
5. Is more interested in sport than in reading. 29.  
Is more interested in reading than in sport. 14.
6. Could be a leader of a work group. 5.  
Could not be a leader of a work group. 32.

III. *Effectivity vs. ineffectivity in work.*

1. Is very clever at school and can nearly always answer the questions. 33.  
Is not at all clever at school and can seldom answer the questions. 18.
2. Is very orderly and careful. 21.  
Is rather slovenly and careless. 6.
3. Always takes his duties seriously and is never noisy in class. 9.  
Hardly ever takes his duties seriously. Often fiddles and titters in class. 24.
4. Is very tenacious. Does not give in when something is difficult. 27.  
Is not tenacious. Gives in easily and often leaves off work before it is finished. 12.
5. Works quickly. 15.  
Works slowly. 30.
6. Is interested in school. Always attends in class. 3.  
Is not interested in school work. Often does not attend in class. 36.

Among these categories – between which of course no definite sharp limits can be drawn – the first and last correspond to the most usual categories of motivation in the investigation discussed above; and the items are in most cases to be found verbatim in the children's motivations. The second category has partly another character and – more independently of the preference motivations – has been made up of statements about social activity or passivity.

In each main category there are six item pairs. One part of each pair refers to positive (or active) behavior, and the other to negative (or passive) behavior.



*Object-Directed Analysis of Socio-Perceptual Selectivity:  
The Pal-Description Device Used for Rating Purposes.*

The pal-description device may be used for rating purposes, and we then consider the socio-perceptual selectivity as at least partly object-determined; or rather, we interpret intersubjective consistencies as signs of object-determination. Epistemologically such a definition of "objectivity" sometimes seems to be the best possible. — Rating by acquaintances has been used in psychological research for many different purposes and in various ways. We may, for example, refer to Husén (13), who used rating with the aid of paired comparisons as a means of obtaining validation criteria. The device used here, however, is in several respects more similar to the "guess-who tests" used in certain investigations of "character reputation" (cf. e. g. 11, 12, 28). The present kind of pal-description device used as a rating technique with children seems to have certain advantages over several other rating methods because of its simplicity and its close relationship to children's own preference motivations.

The following instructions may be given: "On these pages there are several descriptions of boys and girls. Read through them one at a time and check whether any one seems to suit some of the children in your class. Write after each description the names of all the children in your class it applies to. If it does not suit any of them, you do not, of course, write anything. If it suits you, write your own name."

From the primary data two main secondary scores may be established:

$P_i$  = the total number of times a person has been mentioned by others in connection with positive or active behavior;

$N_i$  = the total number of times a person has been mentioned by others in connection with negative or passive behavior.

In a similar way one positive and one negative score for each of the three sub-categories within the form may be established ( $P_1, P_2, P_3; N_1, N_2, N_3$ ).

A pilot investigation has been undertaken, among other things, in order to clarify the relation between this type of rating and the sociometric preference analysis discussed above. The pal-description device was given to a sample of 136 pupils with extreme sociometric positions, among whom were 68 overchosen and 68 overrejected children.<sup>3</sup> Each child received

3. They were selected from 17 school classes: the two most chosen boys, the two most chosen girls, the two most rejected boys, and the two most rejected girls from each class (24 children from each of the 5 age levels 9-13 years and 16 children from the 14 year level). The selection was thus based on extreme values of the  $\bar{p}$ -variable, on

a paper with the names of the eight children from his class, including himself. The instruction given corresponded to the one above, with the exception that the pupils could only choose among these eight children.

Table 1 gives the mean values of each score for the overchosen and overrejected pupils, whereby the values for boys (M) and girls (F) are worked out separately. The table shows a highly significant difference between the two sociometric categories both as regards the total positive and negative scores, and as regards all the subscores except one. The one exception is the  $N_2$  score, where the differences are less clear than for the other scores. The "direction" of the differences is unequivocal, the P-scores being highest for the overchosen, the N-scores for the overrejected. Table 2 gives the mean scores for four sociometric "acceptance levels" in an ordinary school class. Thus in this case we are concerned with a total series of sociometric positions, and not just with extreme cases. The falling tendency of the P-scores, as well as the rising tendency of the N-scores, as we move from level A (most chosen) towards level D (least chosen) is for the most part quite clear. The differences between "much chosen" (A+B) and "little chosen" (C+D) pupils in this class are – as might be expected – not quite as evident as the differences between our extreme cases in Table 1, but nevertheless most of them are highly significant. We therefore have a rather clear statistical basis for what psychologically and logically seems relatively self-evident: the fact that the sociometric status shows a positive correlation with the P-scores and a negative correlation with the N-scores. This means that these two modes of subjective rating – the pal-preference with reference to a group work situation at school and the pal-description in terms of social positivity, social activity and work effectivity – are intimately related, and may, for some purposes, be considered only as different aspects of a complex pal-evaluation totality.<sup>4</sup> But what about the "reality" of the positivity, activity, and work effectivity of our overchosen pupils? Such questions cannot be unequivocally clarified by this device, but have to be further investigated by test situations and interaction situations where the participant pal-perceivers

the one hand, and on extreme values of the  $\bar{n}$ -variable on the other (1, p. 246-247). In some classes where rejections were too few, the least chosen pupils were selected instead of the most rejected.

4. We may consider the correlation between pal-description status and sociometric status as one kind of validity indication for both measures – one of them may be used as a validity criterion for the other. Of course, this only means "external validity", i. e. validity of a couple of hypotheses: that sociometric status may be predicted from pal-description status and vice versa (for this use of the external validity concept, cf. e. g. 29, p. 30-31).

Table 1. An Object-Directed Analysis of the Pal-Description Device. Mean Scores for the Overchosen and Overrejected Pupils.

Score type	Sex	Mean score values		P
		overchosen	overrejected	
P <sub>1</sub>	M	6.74	2.47	<.001
	F	9.82	2.62	<.001
P <sub>2</sub>	M	8.50	2.85	<.001
	F	5.97	2.47	<.001
P <sub>3</sub>	M	10.09	1.44	<.001
	F	9.26	2.94	<.001
N <sub>1</sub>	M	2.03	7.53	<.001
	F	1.65	5.50	<.001
N <sub>2</sub>	M	2.50	4.26	>.2
	F	3.76	6.50	.01
N <sub>3</sub>	M	1.15	9.26	<.001
	F	0.88	7.21	<.001
P <sub>t</sub>	M	25.32	6.76	<.001
	F	25.06	8.03	<.001
N <sub>t</sub>	M	5.68	21.06	<.001
	F	6.29	19.21	<.001

The significance of the difference is expressed in P-values, which have been estimated from four-field tables with the aid of binomial probability paper (cf. 19). The number of pupils was 136 (34 overchosen boys + 34 overchosen girls + 34 overrejected boys + 34 overrejected girls).

Table 2. An Object-Directed Analysis of the Pal-Description Device. Mean Scores for Four "Acceptance Levels" (A, B, C, D) in an Ordinary School Class.

Score type	Mean score values				P
	A	B	C	D	
P <sub>1</sub>	25.8	8.7	7.6	4.1	.009
P <sub>2</sub>	27.3	6.6	4.7	2.3	.001
P <sub>3</sub>	31.2	4.6	5.6	0.6	.06
N <sub>1</sub>	4.7	8.9	11.0	12.6	.12
N <sub>2</sub>	7.4	6.9	13.1	14.6	.12
N <sub>3</sub>	1.8	4.0	13.0	27.8	.001
P <sub>t</sub>	84.3	19.8	17.8	7.0	.003
N <sub>t</sub>	13.9	19.8	37.1	55.0	.003

The P-values have been estimated as in Table 1, whereby A+B has been treated as one category ("much chosen") and C+D as one category ("little chosen"). The class consisted of 35 girls (10-11 years) and was divided into four acceptance levels according to sociometric status (9+9+9+8 pupils).

are replaced by the less situation-involved experimenter-perceiver. But of course we may make some guesses on the basis of our rating: to the extent that we find some degree of consistency between different sets of subjective evaluations – such as those discussed above – we are willing to infer some kind of “objectivity”. We may express this fact better thus: in such cases we may with some degree of probability expect a certain correspondence between our sets of subjective statements and other sets of statements, e.g. the protocol statements of the experimenter. (We may here add that although in our Table 2 the sociometric statements and the pal-description statements were made by the same pupils, this was not the case in Table 1 to the same extent. In this latter the pal-description statements were made by eight pupils, whereas the sociometric preferences were stated by all pupils in the classes. The correspondence between the sets of statements in these cases is therefore more interesting). Hence, we may expect a certain correspondence not only between sociometric status as defined by pal-choices and interaction *quality* as defined by an experimenter-perceiver (cf. the positivity-negativity variable and the effectivity-ineffectivity variable) but also between sociometric status and interaction *quantity* (cf. certain aspects of the social activity-passivity variable), although the latter correlation may perhaps be expected to be somewhat lower (cf. score  $N_2$ , Table 1). Such problems have been discussed by some interaction researchers (cf. 22).

*Subject-Directed Analysis of Socio-Perceptual Selectivity:  
The Pal-Description Device Used as a Test of the Perceiver*

In this section we are not concerned with the rated but with the rater. Whereas we interpreted intersubjective consistencies as signs of object-determination, we now try to interpret intersubjective differences as signs of subject-determination.

The general empirical fact of subject-determined perceptual selectivity – which is one of the fundamental assumptions underlying all “projective” testing – has been verified in many different research situations (cf. e.g. 5, 9, 27) and has also received some attention in the field of social psychology (cf. 8, 25). In the present case we approach our pal-descriptions with questions of the following type: Can we find any *perceiver-determined socio-perceptual selectivity*? For example, is there anything like a sex role determined social rating? Or can we rightly speak of a socio-perceptual selectivity associated with different sociometric positions of the *perceivers*?

From the primary data several scores may be established, e. g.:

Table 3. A Subject-Directed Analysis of the Pal-Description Device. Mean Scores for Overchosen and Overrejected Pupils.

Score type	Sex category	Mean score values			P
		overchosen	overrejected	overchosen and overrejected	
Mp	M + F	7.96	7.93	—	>.2
Mp	M	—	—	9.76	
	F	—	—	6.10	<.001
Mp	M	8.85	—	—	
	F	7.06	—	—	>.2
Mp	M	—	10.68	—	
	F	—	5.15	—	<.001
Fp	M + F	8.10	8.50	—	>.2
Fp	M	—	—	5.55	
	F	—	—	11.06	<.001
Fp	M	6.79	—	—	
	F	9.41	—	—	.007
Fp	M	—	4.29	—	
	F	—	12.68	—	<.001
Mn	M + F	7.16	6.63	—	>.2
Mn	M	—	—	7.19	
	F	—	—	6.59	>.2
Mn	M	7.26	—	—	
	F	6.74	—	—	.15
Mn	M	—	7.12	—	
	F	—	6.15	—	>.2
Fn	M + F	7.54	5.30	—	<.001
Fn	M	—	—	5.88	
	F	—	—	6.96	.15
Fn	M	7.21	—	—	
	F	7.85	—	—	>.2
Fn	M	—	4.56	—	
	F	—	6.03	—	.1

The P values have been estimated as in Table 1, and the population was the same.

Mp = the number of boys (*males*) positively mentioned;

Fp = the number of girls (*females*) positively mentioned;

Mn = the number of boys (*males*) negatively mentioned;

Fn = the number of girls (*females*) negatively mentioned.

These scores were worked out for the sample of 136 pupils with extreme sociometric positions discussed above (cf. table 3).

Certain interesting facts may be observed from the distribution of the values for positive mention. Neither the number of boys (Mp) nor the number of girls positively mentioned (Fp) gives a significant difference between overchosen pupils and overrejected pupils as total categories. On the other hand, there is a highly significant difference for both scores between the sexes. We know from many sociometric investigations that there is often a very small inter-sex choice (cf. e. g. 7). We now see that this choice barrier is also accompanied by a kind of "perceptual barrier": there is a very clear tendency to overrate members of one's own sex – this fact is in accordance with certain earlier findings (cf. e. g. 16). We may perhaps express it thus: we are more generous in our expectancies of other persons' positive behavioral possibilities with respect to our own sex (for recent discussions of the concepts of "behavioral possibilities" and "role expectancy", cf. e. g. 10, 21). The most interesting point, however, is perhaps revealed by an analysis of the overchosen and overrejected children when divided into sex categories. There seems to be a highly significant difference between overrejected boys and girls with respect to the number of boys positively mentioned as well as to the number of girls so mentioned. The perceptual barrier is very clear in these cases. But when we look for this sex difference among the overchosen children, we either find it in a lesser degree (Fp) or – from the point of view of statistical significance – not at all (Mp). Hence we have registered the following important fact: the overrejected boys and girls in this population tend to favor their own sex with positive ratings more than the overchosen do. Or stated in another way: there are facts which indicate that the overchosen may be less sex-role-determined in their positive ratings; that their socio-perceptual selectivity is not so restricted to their own sex.

The analysis of negative mention gives less clear findings. Most interesting perhaps is the fact that we do *not* find any tendency to overreject members of the opposite sex – which we perhaps would like to infer from the principles above. The proper field of both positive and negative "perceptual choices" at this age seems to be one's own sex, although the difference is much more significant with respect to the positive.

## SUMMARY

This paper gives a methodological discussion of choice-motivations in sociometric preference analysis and other similar pal-description statements and illustrates a two-way analysis of such statements: (a) an object-directed analysis in terms of pal-perceptions as object-determined; and (b) a subject-directed analysis in terms of pal-perceptions as subject-determined.

The first section is devoted to general considerations about the difficulties in the analysis of choice motivations. It is concluded that although we cannot always rely on a direct "text interpretation", we may perhaps make some use of a prudent "context interpretation". Regardless of whether the communications are "true" or "false", they are behavioral units of the communicator, and some interesting relation between these and other units of behavior may perhaps be found.

In order to clarify some aspects of these problems it seemed to be practical to start not with the spontaneous choice-motivations, which are sometimes too complex, but to use similar statements in a special pal-description device. The construction of such a device on the basis of choice-motivations is illustrated, and an empirical demonstration is given of an object-directed analysis of socio-perceptual selectivity. The pal-description rating status is found to correlate significantly with the sociometric status as here analyzed.

In the final section a subject-directed analysis of socio-perceptual selectivity is given, i. e. the pal-description device was used as a test of the perceiver. Whereas we earlier had interpreted inter-subjective consistencies as signs of object-determination, we now tried to interpret inter-subjective differences as signs of subject-determination. From the empirical data it was seen that: (a) there is a very clear tendency to perceive members of one's own sex in positive roles more often than members of the opposite sex; (b) there is no corresponding tendency to perceive members of the opposite sex more often in negative roles; (c) overrating of one's own sex is greater for overrejected pupils than for overchosen pupils, i. e. the overchosen children seem to be less sex-role-determined in their pal-descriptions.

Pal-description devices of this kind — many variants may be constructed according to the specific conditions of the research problem and the intended population — may be of use in at least two respects: (a) they may show that the difficulties concerning the "objectivity" of pal-description statements may be made psychologically fruitful when given a subject-directed analysis; (b) they may be used in their own right for rating purposes in order to validate other reputational measures, such as the sociometric choice technique.

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## SOCIAL CLASS, INTELLIGENCE AND THE VERBAL FACTOR

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Class differences in intellectual ability present an intriguing problem of interpretation. The facts are clear. Whenever individuals, children or adults, are given tests of the conventional type, those with a better background in terms of occupation, education, or economic standard, score higher on the average, than those from lower strata. There is, of course, considerable overlapping, and some individuals from the lower rungs of the social ladder will achieve higher scores than most or all members of the upper strata. The general trend, however, is unmistakeable. Numerous studies in many countries point to this being the general pattern, at least in the Western World (1, pp. 787-836; 2, pp. 10-15).

These findings have been interpreted in various ways, depending on the underlying conception of intelligence. If intelligence, as measured by conventional tests, is regarded as "innate", i. e. mainly determined by hereditary factors, it follows that the existing class-system has a biological basis. Nowadays, few sociologists would unhesitatingly subscribe to this view. The perennial question, heredity or environment, needs much conceptual refinement before it can meaningfully be answered.

Perhaps the most provocative answer has been given by a group of sociologists and psychologists who have tried to determine the social role of tests and other measures of ability. As given by Lundberg (4, pp 452-454) the argument may be paraphrased in the following way. Words like "ability" or "intelligence" acquire their meaning in a social context. High ability is defined as the possession of skills needed or highly valued in a particular society. The Icelander of A. D. 1000 had to be a good swordsman and a discriminating judge of poetry. Presumably, his proficiency in handling mathematical symbols was of much less account. In a preliterate culture, still more different from the modern European, the hierarchy of skills would probably be even less like ours. It is at least doubtful whether there are any measurable abilities of importance in a wide variety of cultural settings. In our own society, certain individual capacities are conducive to good adjustment and prosperity, while others seem to be of no

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consequence. Tests of general ability, or intelligence, have to be constructed in such a way that the relevant skills are tapped to a considerable degree, and the irrelevant ones not at all, or to a small degree only. In other words, the tests are constructed so that those who actually adjust well, or are succeeding, score higher on the average than those who do not adjust successfully. It is hardly surprising that people with a better job and higher income come out better on intelligence tests than the rest.

Two things may be remarked about this line of reasoning. On the one hand, it has much to commend itself. In fact, it will be the main purpose of this paper to point out how tests of intelligence are given this particular slant as well as some of the implications for the study of the structure of ability. On the other hand, the argument is not quite conclusive. Some further assumptions have to be made before the inevitability of class differences follows. Granted that tests of general ability will favour those who have actually adjusted successfully to the demands of society, it is not self-evident that their children should fare better than the children of less successful parents. Here, a mechanism of transfer of intelligence has to be assumed, either of a hereditary nature, or dependent on early environmental influences. Empirical parent-child correlations found in countless studies prove that such a mechanism exists. Furthermore, if we look at the history of mental measurement, the guiding criterion most commonly used in test construction seems to have been scholastic success, and not later adjustment or social success in general. If this be true, it follows that tests will differentiate between social classes to the extent that scholastic success and social success in general are related. This assumption is very plausible, although the question is complex and needs more study. On the whole, however, the findings of intellectual differences between social classes seem to be somewhat less trivial than the argument quoted above implies.

What we need is a more detailed study of the origin and nature of the social "bias" of conventional intelligence tests. The four factors mentioned below seem to be of particular importance. The classification cannot be regarded as logically perfect, but will serve our purpose.

1) *Factor of definition.* The decision that "intelligence" should be defined in such a way for instance that verbal tests can be used for measuring it, but not tests of manual dexterity or bodily strength, entails certain class differences.

2) *Factor of motivation.* Children or adults from the upper strata may be more likely to find the test situation stimulating and inherently interesting than do individuals from lower strata.

3) *Factor of familiarity.* Even when the general type of tests to be used has been settled, some items may discriminate more than others because the content is more familiar to people with a certain background. The use of foreign words, or words denoting certain articles of luxury in a vocabulary test, might be a case in point.

4) *Factor of norms.* Given a set of test items, what answers are to be regarded as "correct" or "intelligent" remains to be decided. Here, too, certain groups may be favoured by their particular standard being accepted as the norm.

In what follows, only the latter factor will be discussed in any detail. This must not be understood to mean that it is the most important of the four. The opposite is probably true. It is a fair guess that the factors of definition, motivation and familiarity operate in conjunction in a number of intelligence tests, while the factor of norms is a source of class differences in only a limited number. It so happens, however, that empirical data to hand have a direct bearing on the latter point.

#### VERBAL ABILITY AND CLASS

In a sense, verbal tests are the *bêtes noires* of mental measurement. Judged by the statistical criteria of reliability and validity, tests such as "sentence completion", "opposites", "analogy", and "vocabulary" appear equal or superior to most other types of tasks as measures of intelligence or general ability. Items or scales of the verbal type are included in most intelligence tests in current use. At the same time, they seem open to serious criticism, particularly if intelligence is conceived of as "innate" – something relatively independent of environmental influences. Education, to name one factor, would seem to affect the results to a considerable degree.

It is instructive to regard various types of tests against the background of the four sources of class differences previously mentioned.

By defining intelligence or general ability in such a way that verbal tasks become applicable and valid measures, the upper levels of the social hierarchy are favoured. It may plausibly be argued that for upper and middle class persons, language plays a far more prominent role as an instrument of adjustment than it does in the working class. This, however, appears to be equally true of certain other systems of symbols. For all we know, the factor of definition may favour the same groups even more in the case of tests involving numerical symbols, or purely formal (logical) relations between various entities. Verbal tests are not necessarily the most abstract of those currently used in mental measurement.

If verbal instructions to those taking the test are crucial parts of the

whole procedure, then every test, regardless of its character otherwise, may become verbal to a certain extent, and some groups accordingly favoured.

Generally speaking, the factor of motivation can be expected to work in the same direction as the factor of definition. The test-situation should appeal more to persons coming from a sub-culture where competition and individual achievement are stressed, and, if some sociologists are to be trusted, middle and upper class homes foster such attitudes to a high degree (cf. 2, pp. 20-21).

Furthermore, tests of verbal behaviour should be more interesting to those for which such behaviour plays a vital part in every-day adjustment.

As to the factor of familiarity, it is easy to see how it might affect scores on verbal as well as other kinds of items. Certain words may be better known in some groups, and less or completely unknown in others.

For the three first factors we may then conclude, that they will tend to favour persons from the upper strata of the existing class-system. On a priori grounds, it seems likely that their joint effect will be considerable in the case of tests involving the use of verbal material, though not necessarily larger than for certain other types of tasks of a symbolic and abstract nature. The factor of norms, on the other hand, would seem far more likely to influence verbal tests. To some extent, social classes represent sub-cultures within our own society, and it is quite possible that they differ in language, words in common use, their meaning, sentence construction, and pronunciation. Upper strata may score higher on verbal tests partly because it is their usage that is accepted as the norm. This is less likely to be the case in certain other varieties of tasks, for instance those involving numerical or purely logical relations. There may be a language specific to certain groups in society, but presumably not a logic, or a set of mathematical rules characteristic of one class.

On the whole, empirical data seem to bear out the expectation that verbal tests will give particular large differences between social classes (1, pp. 806-807, 828-830). A recent study by Eells (2, pp. 81-357) illustrates this point. Several intelligence tests, containing altogether many hundreds of items, were given to some 5000 American city pupils of nine, ten, thirteen and fourteen years of age. The social status of the parents was determined, and a high status group and a low status group formed. Different tests and items were compared with respect to the difference in achievement between the two groups. Items that were verbal in terms of the symbolism used and the tasks required tended to show the largest differences.

## A STUDY OF RESPONSES TO A VERBAL TEST

The existence of class differences in scores and response patterns on a verbal test can be demonstrated by means of the group test used in Sweden on the young men registering for military service. The test consists of four sub-scales, viz.:

- 1) *Synonyms*; the nearest equivalent to a given word is to be selected from five possibilities.
- 2) *Concept discrimination*; one of five given words is to be crossed out as differing from the rest in some respect.
- 3) *Number series*; in each item a number series is to be continued with the next number determined according to the system characteristic of the particular series.
- 4) *Matrices*; out of six pieces one is to be selected as fitting into a larger pattern.

It will be seen that these are not unusual tests. Identical or similar scales are used in many places for selection and diagnostic purposes. In terms of reliability and correlation with officers' ratings of the men's ability, or with school marks, these scales do not appear to differ from conventional intelligence tests (3).

A random sample of the conscript population of 1948 was used. For 3 of 21 registration districts the data were incomplete, and the test booklets from these 3 districts could not be used. The final sample, therefore, is less representative than the original, but what remains is still sufficiently close to a good cross-section of the Swedish male population to make the results interesting.

The name and occupation of every person tested is printed on the test booklet. By means of the occupational titles the material was grouped into two categories. There is a "higher group" (H-group) consisting of clerical workers, students, and the like, and a "lower group" (L-group) consisting of manual workers. Needless to say, short occupational titles offer only an imperfect indication of social position, and many mistakes have doubtless been committed in this classification process. There are 660 persons in the lower group, and 267 in the upper.

In *Synonyms*, the items are arranged with one key word to the left, and five words to the right, one of which is to be picked as meaning the same, or nearly the same as the key word. The following would be the English equivalent to an item in this test:

Expensive    rich, costly, genuine, invaluable, good.

Let  $p_0$  denote the proportion of all respondents that did not underline

any of the words, and thus did not answer;  $p_r$  denote the proportion who underlined the "correct" word, and thus in the example above "costly"; and  $p_{w1}$ ,  $p_{w2}$ ,  $p_{w3}$ , and  $p_{w4}$  the portion underlining the first second, third and fourth "wrong" answers respectively.

The hypothesis to be tested is that the low group (L-group) and the high group (H-group) differ in linguistic usage. This implies, among other things, that different words are perceived as the nearest equivalent to a given word. This, in turn, means that the L-group and the H-group will differ not only with respect to  $p_o$  and  $p_r$  values (the H-group generally having a higher  $p_r$  and a lower  $p_o$  value), but also with respect to the relative size of the  $p_w$  values. The norms used in evaluating the test will tend to be those followed by the H-group. In this group, therefore, the correct answer also tends to be the most common answer. In the L-group, on the other hand, some items can be expected to elicit the choice of one of the wrong alternatives more frequently than the "right" one. In such cases the two groups may be said to differ with respect to linguistic norms, or at least linguistic usage.

Table 1.

Item	H-group						L-group					
	$P_r$	$P_{w1}$	$P_{w2}$	$P_{w3}$	$P_{w4}$	$P_o$	$P_r$	$P_{w1}$	$P_{w2}$	$P_{w3}$	$P_{w4}$	$P_o$
1.	.66	.25	.00	.05	.00	.04	.29	.40	.01	.06	.01	.23
2.	.52	.06	.26	.03	.05	.08	.20	.13	.25	.05	.07	.30
3.	.42	.36	.01	.09	.03	.09	.15	.33	.05	.08	.02	.37
4.	.53	.00	.01	.03	.33	.10	.18	.02	.01	.04	.33	.42
5.	.51	.26	.01	.04	.06	.12	.14	.32	.01	.07	.06	.40
6.	.51	.01	.02	.03	.27	.16	.18	.03	.03	.05	.24	.47
7.	.46	.36	.01	.01	.01	.15	.11	.32	.01	.03	.02	.51
8.	.37	.03	.06	.37	.01	.16	.08	.04	.05	.37	.02	.44
9.	.32	.10	.04	.22	.08	.24	.08	.06	.16	.16	.07	.57
10.	.38	.13	.10	.10	.04	.25	.07	.12	.12	.07	.04	.58
11.	.28	.28	.21	.00	.01	.22	.10	.10	.12	.01	.02	.56

These expectations were fulfilled for 11 items out of 40 in synonyms. These items are all from the difficult part of the test. The different  $p$  values for each group are given in table 1. The items have been renumbered from 1 to 11.

It has to be remembered that the test was not designed to reveal social differences in linguistic usage, and that it does so only in an imperfect way. The full extent of such differences might have been more faithfully reflected if the task were to write without restriction the nearest equivalent to the key word. In the present case, the respondents were confined to an artificially delimited domain consisting of five given words.

That the harder items in synonyms do give rise to social differences of this type is, of course, no coincidence. There are only a few ways of making a test of this character difficult. One is to include words with a foreign origin, in Swedish usually a word with a Graeco-Latin root. Such words have a meaning which often is not known except by specialists in a certain field, or persons with an extensive formal education. Because of that, they are normally avoided in a test intended for the general population. The other way is the one actually followed. The words used are all from Swedish roots, although some of them are a trifle literary or archaic in style. The items are rendered difficult by introducing among the alternatives at least one which is not the closest equivalent according to dictionaries and expert opinion, but may appear so. In the case of five (maybe six) of the eleven items, the wrong alternative (distractor) picked most commonly by the L-group is a word often used together with the key word, although not a synonym according to dictionaries and experts. "Safe and sound" would be a very crude English illustration of this kind of relation. Stereotyped expressions used in newspapers and common speech seem to play a part. When the Swedish word approximately equivalent of the English "insolent" is picked as the synonym of the approximate equivalent of the English "repulsive", the most plausible explanation seems to be that these two words are often used to describe the same person. In the L-group the Swedish words for "injury" and "accident" are seen to be very close in meaning; and here again the explanation appears to be that these two words often occur together, e. g. in newspapers. In other cases, a partial similarity between the key word and the selected synonym in terms of spelling and pronunciation is decisive, for instance the fact that the two words have one or two syllables in common.

The fact remains, however, that of the two groups, the higher tends to pick the word designated as the correct one, while the lower elects for a "close" but not the "best" alternative. Whether this proves a difference in norms between the two groups depends on our conception of linguistic norm; apparently it does prove a difference in linguistic habits.

The rules for evaluating the test do not explicitly depend on the usage in the upper group, but rather on what might be termed "expert opinion". That is to say, the right answer is given by certain dictionaries or books of reference, written by reputable linguistic scholars. There is no reason why their opinion of correct usage could not differ on certain points from the custom prevalent in the upper group. On such points they may be guided by historical or linguistic considerations alien to educated but linguistically untrained people in general. It is a safe assumption, however,



that in general, experts will hesitate to condemn as ungrammatical or incorrect word usage or sentence construction common among their educated acquaintances. The upshot is that the upper groups gain the advantage of *their* norm or usage being accepted as *the* norm or *correct* usage.

It is perhaps natural to see this as an "unfair advantage" for one group, and to seek a way of avoiding it. In the present case, this effect was by no means desired when the test was constructed. But there is no easy way round the difficulty. Some items have to be hard, otherwise the test will not differentiate satisfactorily. Other methods of achieving difficulty are also objectionable from the same point of view – for instance the introduction of technical or scientific terms. The whole notion of a "fair" intelligence test is a very obscure one. Whoever asserts that tests should be fair, and that some advantages gained by the upper groups are unfair, in reality has assumed the difficult task of defining, extra-operationally, what "intelligence" is. If sociologists and psychologists want to criticise our system of social stratification and class values they should be free to do so, but taking current intelligence testing as the target instead of the class system can only confuse the issue.

It is not surprising to find that the two social groups here distinguished differ more if the scores on synonyms are used as the basis for comparison than they do on the other three tests. In table 2 mean scores are found for the two groups ( $M_H$  and  $M_L$  respectively) as well as critical ratios for the difference between the groups (CR). The differences are highly significant for all the tests, but are largest for synonyms.

At the same time, synonyms seems to be a more valid test of general ability and military proficiency than the other three scales, if officer's ratings are accepted as criteria (3, pp. 58-64).

Table 2.

	$M_H$	$M_L$	CR
Synonyms	26.8	15.3	17.7
Concept discrimination	24.8	16.6	15.5
Number series	22.1	12.0	15.2
Matrices	24.6	15.7	15.2

The result would probably have been much the same with success in secondary school as the criterion. Because they involve obvious "social biases", the cynic might conclude that verbal tests will more quickly and better do what any intelligence test can do, namely grade people according to class-determined value standards.



## A POSSIBLE REINTERPRETATION OF THE VERBAL FACTOR

So far, the term "verbal" has been used somewhat vaguely to designate certain varieties of tests. From now on, it will be necessary to distinguish between two meanings of the word. On the one hand, the term may be applied to a test because the material contained in it seems mainly verbal, and because we judge, on a priori grounds, that it will tap the ability to handle the signs of language. Often nothing more than a common-sense decision is involved; a test consisting of opposites is called "verbal" simply because it has something to do with words, while a test requiring the completion of number series is characterised as "non-verbal", because words and the meaning of words is less ostensibly involved. Occasionally this a priori designation will be based on a more ambitious psychological analysis of the function involved. On the other hand, we have the a posteriori or statistical definition. Here a test is classified as "verbal" if it correlates to a given degree with certain other tests, or if a statistical factor analysis proves achievement on the test to be determined largely by a factor which we, on the balance of evidence, agree to call "verbal". Needless to say, the psychologist will always be concerned with establishing a connection between those two senses of "verbal", i. e., with showing that tests which appear on common-sense grounds to be verbal, also show themselves to be determined by a verbal factor. That this, broadly speaking, will be true is self-evident. The verbal factor will be found to be of considerable importance in a number of tests involving verbal material; otherwise the factor would not have been so designated. This trivial general connection leaves room, however, for much variation in detail.

Now, because tests involving verbal material ("verbal" in the a priori sense) are as a group likely to exhibit larger differences between social classes than most other types of tests, every other test which also happens to introduce a considerable difference between social classes will be found to correlate fairly highly with verbal tests, and will, very likely, come out as largely verbal in a statistical factor analysis.

Consider the case of the two groups of people, differing in nationality and linguistic background. Both groups are given a battery of tests. In this battery there are a few tests which involve the handling of words and verbal material, and one which is in reality a test of history, and familiarity with certain national symbols and traditions. Let us first make the somewhat unrealistic assumption that both groups, on the average, acquit themselves equally well on all tests. Computation of intercorrelations shows, we may also assume, that the tests based on verbal material tend to be interrelated statistically. They correlate only moderately with the history test.

Let us now change the situation by making one of the groups linguistically and ethnically dominant. The verbal tests are based on the language of the members of this group, and the history test concerns *their* history. The dominating group consequently obtains an advantage and the other group a disadvantage on verbal tests and the history test. Group averages differ on these tests. The matrix of inter-correlations has now been changed. Most important, the history test has been pulled closer to the verbal tests in terms of inter-correlations. How much closer depends, of course, on the amount of "group-handicap". If this is great, we may find that the history test correlates almost as highly with the verbal tests as they correlate with each other. The history test might quite possibly come out as largely verbal if a statistical factor analysis were performed.

This, to be sure, is an over-simplified picture of what actually happens when people from various social strata take intelligence tests. But we have seen that tests built on verbal material usually differentiate between social groups more than do other types of tests. One of the reasons for this – the factor of norms – has been discussed more fully and demonstrated in a previous section of this paper. And it remains true that if a particular category of human behaviour is influenced by the class-position of the individual, then an index or measure of that behaviour will be correlated with scores on verbal tests, or more highly correlated with such scores than would have been the case in the absence of the social factor.

Tests of school achievement (teachers' ratings or scholastic merits) seem to be a case in point. It is reasonable to assume that many factors play a part in the progress of a child at school: intelligence, good looks, nice manners, parents' insistence on achievement, etc. All or most of these factors will tend to pull school success as a variable closer to the scores on verbal tests. It will be seen, then, that behind the factor loadings, and factor designations resulting from the statistical analysis of intercorrelations, may be hidden an extremely complex reality. The "verbal factor" may be a joint product of various psychological and sociological influences, whose separate product of various psychical and sociological influences, whose separate contributions it is impossible to assess with any precision.

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# DIMENSIONS OF EMOTION

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## PROBLEM

In spite of the complexity of emotional experience and the resulting vagueness of any verbal definition of emotion, the problem of emotional dimensionality remains meaningful. We all agree that sad, gay and happy denote emotional states which are rather clearly distinguishable. Just how many dimensions are necessary to account for the main differences between these and other presumably well-known emotions, and which are these dimensions?

This is a problem which can be attacked experimentally. The present writer is entirely convinced that it cannot, at present, be solved in any definite way. But it may be possible to present partial solutions, to locate tentatively some of the vectors of the reference system we need.

This is one attempt in this direction.

## METHOD

The method of similarity analysis was developed by the present writer as a means of analyzing the dimensionality of qualitatively different experiences. It has been briefly described in several recent papers (1, 2, 3). Its main features are as follows:

Stimuli are presented two at a time or in succession. The subjects have to rate the degree of subjective similarity between stimuli. In this way, a similarity score is obtained for every pair of stimuli. The matrix of similarity scores is factored in the same way as a matrix of correlation coefficients. The final (rotated) factor matrix indicates the relative contributions of the more or less primary experiences to the more complex ones under investigation.

This method has been used in several investigations (1, 2, 3). It was, in fact, first applied to emotion (1, 2). That study was mainly intended as a preliminary experiment with the method, but the results were rather clearly interpretable and have been used as one of the bases for the selection of stimuli in the present investigation.

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## THE EXPERIMENT

A list of 23 Swedish words were prepared to represent a wide variety of emotional states. The selection of words was aided by the previous investigation mentioned above. The factors of pleasure and discomfort turned out to be over-represented in the first experiment. Consequently, some of these words were excluded from the list. Other words were added to elucidate some of the six dimensions first isolated, or to make possible a tentative definition of some additional factors. The complete list of 23 words appears in a subsequent section.

Two forms of this list were prepared. One was arranged in (Swedish) alphabetical order, the other in reverse order. The two forms were rotated among the subjects.

The subjects were instructed to rate the degree of qualitative similarity between the emotional states represented by the stimulus words. The rating scale ranged from 0 (not at all similar) to 4 (identity).

The subjects were 168 students of psychology.

## RELIABILITY OF RATINGS

The agreement between subjects has been studied in this investigation. The whole group of 168 subjects was divided in two halves, each of 84 subjects. In each of the sub-groups there were 42 male and 42 female subjects, and also equally many of the two forms of the stimulus list (alphabetical and reverse). Apart from this equating of the two groups, the division was made in a random way.

The mean similarity matrix was obtained for each of the two sub-groups. The correlation between the two matrices, each containing 253 entries, was + .9932. When stepped up by the Spearman-Brown formula, this yields a reliability coefficient for the ratings from the whole experimental group of + .9966. In the same Spearman-Brown sense, the correlation between two average observers is + .6349.

These figures show:

- 1) That on the whole, different subjects agree rather well on their ratings, which is indirect evidence that the same stimulus word means much the same to them, and
- 2) That the reliability of the group data is very high according to usual standards in biological research.

## ANALYSIS OF DATA

A comparison between male and female similarity matrices did not show any important differences. Consequently, all subjects were treated as one group in the final computations.

The similarity scores were obtained for the whole group of 168 subjects and were reduced to a scale ranging from 0 to 1. This similarity matrix is given in table 1 at the end of this paper.

The similarity matrix was factored by the complete centroid method described by Thurstone (4). The question of how far to continue the analysis was solved in the following way:

Similarity matrices were obtained for both of the two sub-groups mentioned above, and the difference matrix was obtained. Factorization was continued until the distribution of residuals was well within the distribution of these differences between random halves. - This appears to be a somewhat safe practice. It has been applied in all our work with similarity analysis, as no definite criterion for concluding factorization exists.

The orthogonal factor matrix was rotated to simple structure. The rotated oblique factor matrix is given in table 2. The cosines of the angular separations between the vectors appear in table 3, and the inferred similarities between factors in table 4.

In order to save space we reproduce, at the end of this paper, only a sample of the 55 factor plots. The total group of plots, just as this sample, clearly shows a very satisfactory simple structure. There is practically no margin for alternative solutions. - All the rotations were performed blindly.

## FACTORS IN EMOTION

The oblique factor matrix of table 2 has been re-arranged in the table below, where only loadings above .30 have been entered. In this table the stimulus words are grouped according to the main factor involved. With reference to this table, the interpretation of the factors may be briefly given.

*Factor A - Pleasure*

This factor is best defined by happy and glad, and to some degree by gay. Obviously, it is the factor of pleasure to be expected in any dimensional analysis of feeling and emotion. The fact that the two words which have the highest loadings with factor A, also have considerable projections on two other dimensions, is interesting. It will be discussed in a subsequent section.

The Main Factor Loadings

	A	B	C	D	E	F	G	H	I	J	K
happy (11)	.629						.354				
glad (6)	.626				.370						
sad (19)		.859									
depressed (14)		.758									
desperate (5)		.464								.427	
restless (17)			.717								
impatient (16)			.681						.357		
agitated (15)			.677			.567					
irritated (8)			.510						.493		
longing (12)				.733							
want (18)		.482		.630							
desire (2)				.548						.366	
animated (10)					.678						
gay (13)	.373				.626						
anxious (22)			.417			.744					
frightened (4)						.594					
tenderness (23)							.722				
affectionate (9)							.643				
disgust (1)								.681			
rancorous (7)								.670			
angry (3)									.768		
ireful (20)									.594		
benevolent (21)											.507

*Factor B - Discomfort*

Our second factor is also to be expected to appear in any analysis of emotion. Its interpretation is self-evident from the defining words: sad, depressed, and - though less representative - desperate. From the point of view of factorial composition, sad and depressed seem to denote rather homogeneous or pure emotional states, whereas happy and glad represent states of some complexity, in different ways. In other words, our factor of discomfort is defined in closer connection with the experimental stimuli than our pleasure-factor, which represents a somewhat higher degree of abstraction.

*Factor C - Agitation*

The third factor is defined by the stimulus words restless, impatient, agitated, and irritated. The common dimension may be described as restlessness or agitation. This interpretation is strongly supported by the projection of the three last-mentioned words on other factors (G, H) which are clearly identifiable (see below). - The factor of agitation was mentioned long ago by Wundt as a part of one of his three bi-polar dimensions. His first dimension Lust-Unlust is represented by our uni-polar factors of pleasure and discomfort. The remaining three parts of Wundt's system are not identifiable in our analysis, and they appear to be rather abstract constructs.

*Factor D - Longing*

The fourth factor is defined by longing, want, and desire. The common dimension may be described as longing. Again, this interpretation is strongly supported by the projections of want and desire on two other, independently identifiable dimensions (see below).

*Factor E - Animation*

The interpretation of the fifth factor follows from its definition by the stimulus words animated and gay. The common dimension is liveliness or animation. This interpretation is supported by the second projection of gay on the dimension identified as pleasure. The difference between lively and gay is, of course, the pleasing quality of gay.

*Factor F - Fear*

The defining words are anxious and frightened. The common dimension seems to be fear. The interpretation is supported by the projection of anxious on the dimension clearly defined as agitation. It is further supported by a third defining stimulus word, agitated, which has its second projection on F, its factorial composition being much the same as that of anxious. (The similarity between agitated and anxious is .79.)

*Factor G - Affection*

The seventh factor is clearly defined by the high loadings in the stimulus words tenderness and affectionate, which have no considerable projections on other dimensions.

*Factor H - Disgust*

Our eighth factor is defined by high loadings in the words disgust and rancorous, which have no considerable projections on other dimensions.

*Factor I - Anger*

The ninth factor, again, is defined by high loadings in two factorially rather pure words, angry and ireful. It is also represented by irritated and impatient, which, however, have their main projections on agitation. The common dimension seems to be anger. - It will be noted that the psychological difference between factors H and I is not quite clear.

*Factor J*

The tenth factor is defined by desperate and desire, but both words have higher projections on other dimensions (discomfort and longing, respectively). It may best be described as the factor that makes desperate different from sad, and desire different from longing. It may be elucidated in further research.

*Factor K*

The eleventh factor has but one large loading, in benevolent. It has to be left without interpretation.

Out of the eleven factors, nine may be rather clearly interpreted, although the difference between H (disgust) and I (anger) ought to be further investigated. The two remaining factors could not be defined with any certainty, but they do not appear to be mathematical artefacts.

### COMPLEX EMOTIONS

Our main purpose has been to isolate some factors in emotion, which may be considered to be more or less primary.

Any successful analysis of the dimensionality of a domain will yield a frame of reference which is useful for purposes of description. From this point of view, the complex emotions are of some interest to this study.

According to our analysis, eight out of the twenty-three stimulus words represent emotional states which are especially complex as far as the nine interpreted factors are concerned. The factorial description of these complex emotions, as given by the set of factor loadings, appears psychologically very sound and plausible. Consider, e. g., the stimulus words "glad" (pleasure with some animation), "gay" (pleasure with more animation), and "impatient" (agitation with some anger).



## SIMILARITY BETWEEN PRIMARY EMOTIONS

Table 4 gives the inferred similarities between our factors of emotion. Mathematically, of course, these similarity scores are analogous to correlations between factors in factor analysis of a correlation matrix. Psychologically, however, the interpretation must be in terms of subjective similarity.

The table shows a few larger entries, which all are psychologically very plausible.

The oblique solution was preferred mainly for technical reasons. With this solution, a very satisfactory simple structure is obtained, and the location of the axes is possible practically without any margin of uncertainty. A second, independent set of blind rotations would have yielded practically the same results.

So far, however, there is no really convincing interpretation of this "technical similarity" between primary factors of experience. It may be just technical. In that case, the "real" factors would not show any similarity, but on the other hand they would be located with less accuracy in the analysis. Or the similarity may be "real". This, in turn, might be interpreted in different ways. In some cases it might be due to functional overlap or interdependence of the organic structures involved in the experience. These problems have to be settled, but the way of solving them is not in sight.

In this analysis, the factors of table 2 could be made orthogonal without any radical change of their psychological interpretation. The picture, however, would become a little less clear.

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Table 1. Similarity Matrix

Stimulus	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23
1		.04	.26	.16	.10	.00	.61	.24	.00	.02	.00	.01	.00	.11	.06	.10	.05	.01	.05	.43	.00	.06	.01
2	.04		.02	.10	.10	.09	.10	.24	.10	.10	.55	.05	.06	.12	.22	.22	.32	.05	.07	.04	.05	.22	
3	.26	.02		.15	.19	.00	.37	.55	.01	.06	.00	.03	.00	.20	.14	.33	.15	.05	.07	.72	.01	.10	.01
4	.16	.02	.15		.31	.00	.09	.16	.01	.04	.00	.02	.00	.14	.33	.07	.13	.06	.09	.12	.01	.46	.01
5	.10	.10	.19	.31		.00	.13	.23	.03	.03	.00	.15	.01	.49	.37	.15	.25	.27	.54	.19	.01	.41	.06
6	.00	.10	.00	.00	.00		.01	.01	.34	.43	.70	.11	.84	.00	.03	.05	.05	.00	.00	.00	.33	.01	.21
7	.61	.09	.37	.09	.13	.01		.27	.01	.03	.00	.03	.00	.11	.08	.11	.09	.03	.07	.48	.00	.05	.00
8	.24	.10	.55	.16	.23	.01	.27		.01	.08	.01	.07	.01	.25	.40	.60	.44	.07	.09	.41	.00	.26	.00
9	.00	.24	.01	.01	.03	.34	.01	.01		.10	.50	.26	.19	.03	.09	.06	.04	.12	.07	.01	.46	.10	.80
10	.02	.10	.06	.04	.03	.43	.03	.08	.10		.21	.05	.57	.00	.17	.19	.29	.01	.01	.08	.12	.05	.04
11	.00	.10	.00	.00	.00	.70	.00	.01	.50	.21		.16	.45	.01	.03	.03	.03	.02	.01	.01	.31	.02	.39
12	.01	.55	.03	.02	.15	.11	.03	.07	.26	.05	.16		.04	.19	.19	.25	.24	.65	.23	.02	.07	.17	.29
13	.00	.05	.00	.00	.01	.84	.00	.01	.19	.57	.45	.04		.01	.03	.03	.06	.00	.00	.01	.26	.01	.09
14	.11	.06	.20	.14	.49	.00	.11	.25	.03	.00	.01	.19	.01		.36	.17	.21	.35	.75	.15	.02	.33	.06
15	.06	.12	.14	.33	.37	.03	.08	.40	.09	.17	.03	.19	.03	.36		.44	.65	.25	.21	.11	.04	.79	.11
16	.10	.22	.33	.07	.15	.05	.11	.60	.06	.19	.03	.25	.03	.17	.44		.64	.17	.09	.29	.01	.31	.02
17	.05	.22	.15	.13	.25	.05	.09	.44	.04	.29	.03	.24	.06	.21	.65	.64		.20	.15	.13	.01	.44	.02
18	.01	.32	.05	.06	.27	.00	.03	.07	.12	.01	.02	.65	.00	.35	.25	.17	.20		.47	.06	.03	.28	.18
19	.05	.05	.07	.09	.54	.00	.07	.09	.07	.01	.01	.23	.00	.75	.21	.09	.15	.47		.12	.04	.30	.13
20	.43	.07	.72	.12	.19	.00	.48	.41	.01	.08	.01	.02	.01	.15	.11	.29	.13	.06	.12		.00	.09	.01
21	.00	.04	.01	.01	.01	.33	.00	.00	.46	.12	.31	.07	.26	.02	.04	.01	.01	.03	.04	.00		.07	.33
22	.06	.05	.10	.46	.41	.01	.05	.26	.10	.05	.02	.17	.01	.33	.79	.31	.44	.28	.30	.09	.07		.13
23	.01	.22	.01	.01	.06	.21	.00	.00	.80	.04	.39	.29	.09	.06	.11	.02	.02	.18	.13	.01	.33	.13	

Table 3. Cosines of the Angular Separations of the Vectors

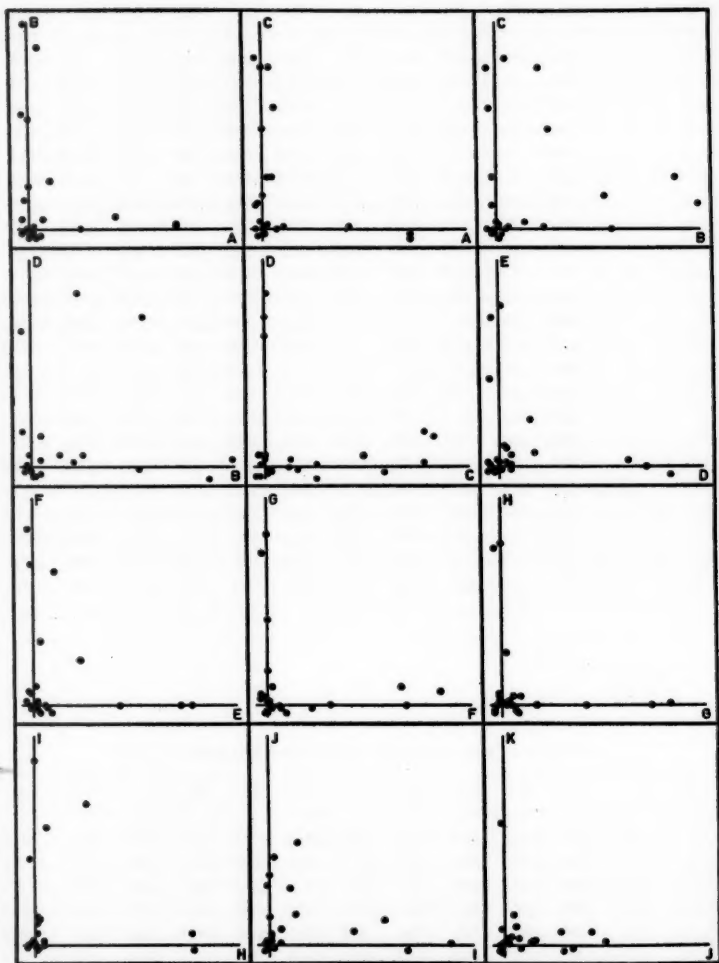
	A	B	C	D	E	F	G	I	I	J	K
A	1.001	.023	-.061	-.065	-.279	-.108	.134	.005	.000	-.005	-.336
B	.023	.999	-.080	-.015	.110	-.108	.061	-.002	-.222	.111	.101
C	-.061	-.080	1.000	-.193	.137	.065	-.009	-.002	.067	-.038	.033
D	-.065	-.015	-.193	1.000	-.058	.002	-.243	-.014	-.007	.016	.152
E	-.279	.110	.137	-.058	1.000	-.069	.073	.000	.007	.032	-.162
F	-.108	-.108	.065	.002	-.069	1.000	.016	-.057	.467	.019	.021
G	.134	.061	-.009	-.243	.073	.016	1.000	-.007	.013	-.121	-.545
H	.005	-.002	-.002	-.014	.000	-.057	-.007	1.000	-.470	-.083	.008
I	.000	-.222	.067	-.007	.007	.467	.013	-.470	1.000	.057	-.012
J	-.005	.111	-.038	.016	.032	.019	-.121	-.083	.057	1.000	.135
K	-.336	.101	.033	.152	-.162	.021	-.545	.008	-.012	.135	1.000

Table 2. Rotated Oblique Factor Matrix

	A	B	C	D	E	F	G	H	I	J	K
1	.004	-.018	-.021	.010	-.005	.045	.000	.681	-.022	.008	.003
2	.003	-.042	.000	.548	.025	-.032	.045	.036	.015	.366	.061
3	-.025	-.017	.097	-.004	.021	.005	.018	-.001	.768	.009	-.003
4	-.008	-.003	.026	-.017	-.008	.594	-.004	.010	.090	.235	.056
5	.000	.464	.139	-.007	.034	.271	-.003	.007	.119	.427	.015
6	.626	.018	-.020	-.037	.370	-.002	.153	.000	.012	.015	.011
7	.005	-.009	-.019	.048	-.010	-.004	-.025	.670	.046	.006	.026
8	.050	-.026	.510	-.023	-.020	.062	-.008	.047	.493	.114	.005
9	.057	.037	-.002	.026	.020	-.022	.642	-.003	.005	.037	.130
10	.017	-.024	.223	.014	.678	-.003	.024	.010	.052	.071	-.011
11	.629	.010	-.042	-.035	.013	-.003	.356	-.007	.018	-.012	-.030
12	.089	.200	.005	.733	-.033	.021	.074	-.028	-.004	.118	.018
13	.373	.048	.013	-.030	.626	-.001	-.019	.003	.008	-.008	.070
14	.036	.758	.216	-.053	.024	.021	-.009	.035	.003	.292	-.006
15	.000	.181	.677	.024	.085	.567	.081	.044	.018	-.013	.008
16	.028	-.040	.681	.153	.058	-.010	-.031	-.019	.357	.056	.033
17	-.028	.040	.717	.129	.197	.193	-.009	.024	.109	.131	.007
18	-.025	.482	-.003	.630	.007	.079	-.031	-.029	-.010	.028	.037
19	-.023	.859	.110	.030	.078	-.030	.032	.015	-.005	.251	-.015
20	-.030	-.010	.003	.050	.053	.004	.017	.215	.594	-.015	-.019
21	.016	.012	.019	-.006	-.006	-.013	.040	.000	.003	-.006	.507
22	.002	.218	.416	.046	-.024	.744	.056	-.023	.008	.046	.079
23	-.020	.122	-.031	.046	.013	.003	.722	.007	-.016	-.019	-.001

Table 4. Inferred Similarities between the Factors

	T <sub>A</sub>	T <sub>B</sub>	T <sub>C</sub>	T <sub>D</sub>	T <sub>E</sub>	T <sub>F</sub>	T <sub>G</sub>	T <sub>H</sub>	T <sub>I</sub>	T <sub>J</sub>	T <sub>K</sub>
T <sub>A</sub>	.999	-.051	.009	.039	.289	-.051	.077	-.011	.003	-.002	.364
T <sub>B</sub>	-.051	.999	.221	.684	-.077	.120	.000	.017	.129	-.097	-.277
T <sub>C</sub>	.009	.221	1.000	.317	-.051	.514	.030	.381	.193	-.009	-.031
T <sub>D</sub>	.039	.684	.317	1.001	.034	.027	.201	-.029	-.157	.096	-.009
T <sub>E</sub>	.289	-.077	-.051	.034	.999	-.005	.038	-.028	-.080	-.055	.248
T <sub>F</sub>	-.051	.120	.514	.027	-.005	.999	-.013	.722	.434	-.018	-.074
T <sub>G</sub>	.077	.000	.030	.201	.038	-.013	1.000	.004	-.013	.061	.504
T <sub>H</sub>	-.011	.017	.381	-.029	-.028	.722	.004	1.000	.280	.088	-.030
T <sub>I</sub>	.003	.129	.193	-.157	-.080	.434	-.013	.280	1.000	.033	-.042
T <sub>J</sub>	-.002	-.097	-.009	.096	-.055	-.018	.061	.088	.033	1.000	-.088
T <sub>K</sub>	.364	-.277	-.031	-.009	.248	-.074	.504	-.030	-.042	-.088	.999



Samples of the factor plots

# MOTION PERCEPTION AND PERSONALITY. I

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To most observers the perceived motion of a single object moving in a visual field increases considerably when a second object, moving with the same speed in the opposite direction, is introduced.

This will be clearly demonstrated if, for instance, two small bright spots are made to move horizontally to and fro with a pendulum motion on a white screen as indicated in Fig. 1. By making successive comparisons between the velocity of these two dots and that of a single dot in an equal field it will be found that most subjects overestimate the velocity of the two dots up to the double. This increase in perceptual velocity when two moving elements occur in the same field has earlier been investigated by one of the writers (2).

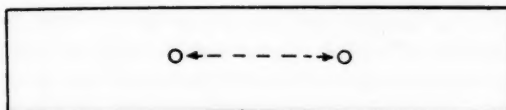


Fig. 1

We can very briefly summarize the relevant principles for this phenomenon in the following way:

The perceived velocity of two dots in a visual field such as the one mentioned above can appropriately be described as the perceptual equilibrium between divergent influences originating from two different frames of reference. The stationary spatial framework, in relation to which we measure the physical speed of the distal stimulus, forms one of these. The other is constituted by the opposite movement of the second object. The motion of one object is thus seen as relative to the other moving object, and the outcome of this influence is a perceptual addition of the physical velocities, a *velocity synthesis* (3, p. 114).

The original study of velocity perception in the relevant constellations was characterized by a rather slight interindividual dispersion of the means obtained by the method already outlined.

Some subjects, however, received scores for velocity perception which

diverged in a striking manner from those for the rest of the group. Their scores indicated that the static frame of reference was dominant even when the scores of the remaining subjects indicated that the influence of the other moving object was completely predominant.

It struck the experimenter as rather remarkable that those subjects (he knew them quite well) who deviated most, experienced distinct difficulties in attaining social contact and also exhibited other pronounced introversive traits. In one case, the personality seemed to be typically schizoid. Thus, these subjects revealed: (a) that they were largely unaffected by the other object in their motion perception (slight velocity synthesis), and (b) that their social contacts were very restricted or that they adopted an attitude of social isolation.

It seemed to be possible to hypothesize some kind of functional relationship between the perceptual and social response observed. Subsequently the writer was able to make certain observations which appeared to confirm this assumption, since two more persons with grave schizoid symptoms showed an extremely low degree of "object dependence" in their motion perception. At this juncture we considered that it was worth while testing the assumption experimentally.

As part of an extensive research project for the investigation of the differential validity of certain clinical tests of cognition and personality carried out at the psychiatric clinic of Uppsala, we have tried to test the above mentioned assumption and to give it a more definite formulation.

As a starting point for our investigation we formulated the hypothesis in the following way: Those subjects who obtain extremely high scores on the velocity synthesis test (= extremely low degree of velocity synthesis) will in the personality description be characterized by traits such as autism or low degree of social contact.<sup>1</sup>

As a consequence of this hypothesis, the personality traits mentioned were given special attention in the personality analysis of our subjects.

#### APPARATUS AND PROCEDURE

Our subjects (Ss.) sit in a chair facing a homogeneous screen ( $50 \times 50$  cm) at a distance of 75 cm. Two bright dots about 3 mm in diameter are seen in the central part of this screen. These dots move to and fro along a common horizontal track in a simple harmonic motion (a pendulum motion, see Fig. 1). Their phase relation is  $180^\circ$ , that is, they turn at the same moment

1. Although these symptoms belong to the more inclusive syndrome of "schizoid personality", the relative vagueness of the latter concept makes it unsuitable for our purpose.

at each end of the common track, cross each other in the middle of the track, turn again at the opposite end, etc. The motions of the dots have the same frequency, viz. 0.7 cycles per sec. The length of the track is 20 mm, which covers a visual angle of about 1.5 degrees.

S has a switch in his left hand. When he presses this switch the two dots disappear, and he now sees a single dot moving along the same horizontal line. This dot has the same frequency as the two first dots, the length of the track, however, is now adjustable. By turning a knob with his right hand S is able to regulate the length of this track and thus also to regulate the velocity of the dot.<sup>2</sup>

S was instructed to adjust the velocity of the single dot by making a series of comparisons until it appeared equal to that of the two dots. There was no time restriction whatever. S was allowed to adjust the velocity up or down the scale as he chose, but every adjustment had to begin from one of the termini of the velocity scale. The starting points, which were rotated according to the scheme ABABA, represented velocities which were definitely too high or too low for each S. The scale runs from 50 to 210 units. 200 on the scale means that the velocity of the adjustable dot is equal to that of either of the two standard dots. Thus, this value indicates complete absence of velocity synthesis (VS). A VS score of 100, on the other hand, indicated complete VS, which is a velocity double that of the standard dots. <sup>VS</sup>

No instruction about fixation is given: S is allowed to look freely at the moving dots and to follow his spontaneous perception attitude.

Ss who observed the change in the length of the motion track in the adjustable field were told not to depend on the length of track as a criterion of equality, but to use only the immediate impression of velocity in the fields.

Two groups of subjects were investigated. The first group (N=56) was chosen at random from the male and female patients of the Psychiatric Clinic at the University Hospital of Uppsala. The ages of Ss in this group varied from 20-67 years, the mean age being 37.56 years. The second group (N=42) was made up of 20 conscripts who were 20 years old and 22 male out-patients from the surgical department of the hospital, who were suffering from minor wounds, fractures, etc. The age of the latter group varied from 18-62 years, and the mean was 36.25 years.

All Ss were interviewed and examined by one of the authors who is a senior psychiatrist at the clinic. The latter, of course, had no know-

2. For description of an apparatus see (2). The stimulus pattern can also be readily produced on the screen of a cathode ray oscillograph with two beams (or an oscillograph + an electronic switch) by means of some rather simple additional devices.

ledge of the patients' VS scores at the time of examination. Short diagnostic summaries were made of the case-reports. These summaries were then sorted into three groups referring to a hypothetical continuum of different "social attitudes":

- groups*
- I. Autistic, withdrawn, reserved, seclusive, retiring.
  - II. Undecided, ambivert.
  - III. Sociable, open, frank, outspoken, expressive.

The terms assigned to the three different categories are those used in the descriptive summaries.

## RESULTS

The experimental data are given in Table 1, and also in graphic form in Fig. 2.

It is evident from Table 1 that there is a marked tendency for Ss characterized as "autistic", "withdrawn", "retiring", "seclusive", etc., to cluster in the upper part of the VS-scale. This tendency for the "sociable", "frank", "open", "expressive" Ss to cluster at the lower end of the VS-scale is more accentuated in the normal than in the hospitalized group.

As an exploratory test of the statistical significance of these tendencies an analysis of variance was made in accordance with a  $3 \times 2$  factorial design. As can be seen in Table 2, where the complete analysis of the VS-variance is given, a highly significant value of  $F$  ( $P < .001$ ) is obtained for the variation between the three different diagnostic groups.

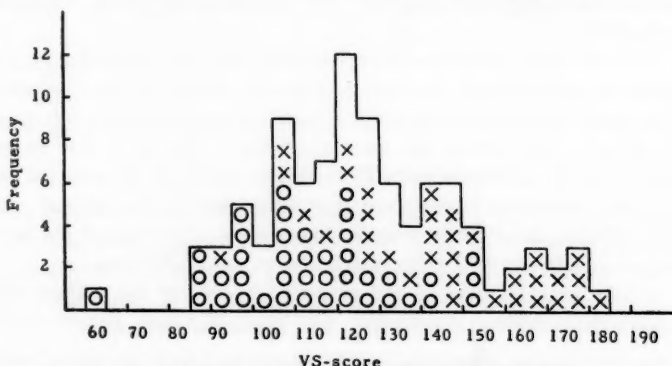


Fig. 2. Distribution of the three diagnostic categories on the VS-scale. Crosses refer to category I, circles to III. The unfilled space refers to II.



Table 1. VS-scores for the three diagnostic categories.

VS score	Autistic		Undecided		Sociable		Total
	Norm.	Hosp.	Norm.	Hosp.	Norm.	Hosp.	
60- 64	-	-	-	-	1	-	1
65- 69	-	-	-	-	-	-	-
70- 74	-	-	-	-	-	-	-
75- 79	-	-	-	-	-	-	-
80- 84	-	-	-	-	-	-	-
85- 89	-	-	-	-	-	3	3
90- 94	-	1	-	-	2	-	3
95- 99	-	-	-	-	3	2	5
100-104	-	-	1	1	1	-	3
105-109	-	2	-	1	2	4	9
110-114	1	-	-	1	2	2	6
115-119	-	1	1	2	2	1	7
120-124	1	1	3	1	2	4	12
125-129	2	2	2	1	-	2	9
130-134	-	2	2	1	-	3	8
135-139	1	-	-	2	1	-	4
140-144	1	3	-	-	-	2	6
145-149	2	3	-	1	-	-	6
150-154	-	1	-	-	2	1	4
155-159	1	-	-	-	-	-	1
160-164	2	-	-	-	-	-	2
165-169	1	2	-	-	-	-	3
170-174	1	1	-	-	-	-	2
175-179	2	1	-	-	-	-	3
180-184	-	1	-	-	-	-	1
N	15	21	9	11	18	24	98
M	149.0	140.3	122.6	123.4	110.9	116.6	
SD	20.84	24.20	7.17	13.98	21.93	18.41	

Table 2. Analysis of variance of VS-scores for six groups of subjects.

Source of variation	Sum of squares	df	Mean square	F	P
1. Between diagnostic categories	17598	2	8799	22.504	< .001
2. Between norm. and hosp.	1	1	1		
3. Interaction 1 $\times$ 2	992	1	496	1.268	
4. Within groups	36008	92	391		
Total	54599	97			

In order to complete the analysis of variance, the significance of the difference between the VS-means of the two extreme diagnostic categories was tested.

Table 3 shows that the difference between the means of the two extreme diagnostic categories is significant at the . 1% level both in the normal and the hospitalized group.

*Table 3. t-test of significance of the difference between the VS-means from the two extreme diagnostic categories (autistic - sociable).*

Group of Ss	Diff.	t	df	P
Normal	38.11	5.079	31	< .001
Clinical	23.75	3.857	43	< .001
Total	29.80	6.169	76	< .001

#### *The reliability of the VS-scores*

One of the more pertinent methodological problems is concerned with the reliability of the VS-scores. As was mentioned before, the individual VS-score was ascertained as the mean of ten trials.

One approach to the question of reliability is to compute the standard error of the mean of the trials for each S. The mean standard error for our normal Ss was 5.63 and for the clinical group 9.11. Thus the clinical Ss were, in general, more indeterminate and less precise in their matching performance than the normal ones.

Another approach to the question of reliability was to re-examine our clinical Ss, first after an interval of 6 hours and then a second time after an interval of 24 hours. The control-group was re-examined after an interval of 22 days.

The coefficient of stability between the first and second scores was .81, and between the first and third scores .83. For a group of 20 control Ss who were re-examined after an interval of 22 days the coefficient of stability was .79. From these three instances it seems fairly reasonable to conclude that a representative coefficient of stability, for intervals varying from 6 hours to 22 days, for the average of ten trials is .80.

#### DISCUSSION

1. When conducting our analysis of the intraindividual scatter in connection with the discussion of the reliability of the measurements, we came across a phenomenon which may be of some intrinsic interest. We

found that in their matching performance, some of the clinical Ss were greatly influenced by the initial velocity of the adjustable moving dot. It was possible to group their scores for the ten trials in two discrete series; one, originating from adjustments starting from a low velocity, and the other, from adjustments starting from a high velocity. There could be a considerable span between the means of these two groups of scores (in one case, for example, 73-178), while the dispersion within the two groups might remain normal. It is evident, that the mean for the ten trials, as computed in our investigation, must, in such cases, have a value which lies somewhere between the two extremes, and that the dispersion around such means must be very great.

We have examined the clinical data for thirteen of the Ss who were the most extreme in this respect. Our impression is, that "a passive, dependent social attitude" may well be taken as a common denominator of the diagnoses.

Both the tendency to be influenced by a starting point in a perceptual matching situation (a "lagging" tendency) and the personality traits mentioned above, seem to us to indicate a possible connection with the leveling-sharpening dimension as hypothesized by Klein (1, 4).

2. The VS-scores for the psychiatric patients refer to their pre-treatment period. Some of these patients who later received electroshock or insulin treatment, have been "followed up" by means of successive VS-retesting. Our preliminary impression based on these cases is that neither of these treatments alters, in any conclusive way, the VS-level.

3. The results of our investigation have proved a very good confirmation of the hypothesis which we intended to test. We wish, however, to make it quite clear that the work reported here represents only an introductory, rather exploratory, part of a more comprehensive study of the functional relations between motion perception and personality traits. Consequently, we shall refrain in the present report from explicitly discussing our results as related to theory.

We will only state here, that as a starting point for the forthcoming work, we have tentatively hypothesized the existence of an analytical, isolating or self-oriented perceptual attitude behind the extremely high VS-scores. We also assume that this attitude is a formal pattern which we may expect to find in other suitable perceptual situations as well as in some forms of intellectual, emotional and social behaviour.

The first part of this set of assumptions has a rather stable basis. We know that the basic relations between a stimulus situation built up of several elements in simple harmonic motion and different forms

of perceptual responses to this stimulation are all consistent from a mathematical point of view, and can be described from the standpoint of psychophysics. The response type may vary, e. g., with a change in fixation attitude, but in any case it is a mathematically correct representation of the stimulation (3).

The two additional assumptions are merely reformulations of the common general assumption on which all sound theorizing about perception-personality relations is based.

Thus, we definitely base our work on a psychophysical relationship between stimulation and perception, and not on a subjectivity of perception which is all too often presumed to exist in this field.

#### SUMMARY

Experiments in the field of motion perception had indicated that there were good reasons for assuming a correlation between an analytical or isolating attitude in perception and a tendency to social isolation. The hypothesis advanced was: Those Ss who obtain extremely high scores in the velocity synthesis test, will be characterized, in the description of their personality, by traits like autism, or a low degree of social contact.

This hypothesis has been tested and the result may be taken as confirming the validity of the hypothesis.

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## ON PERSPECTOID DISTANCES

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When we move around we can experience peculiar apparent movements of the objects surrounding us. If, for example, we take a walk in a forest, we can, with a particular attitude, see something like movements of the trees in relation to each other. If we interest ourselves for two trees, for instance, then while walking past them we can often see the one as being behind the other, and, as we continue moving, it appears a little to the right of the other, gradually receding more and more from it. If we stand still we can interest ourselves for a certain "recedence in the field of vision" of the one tree with regard to the other, and this recedence must, of course, not be confused with the true distance between the trees, which is constant. This "recedence in the field of vision" is maximal when one is standing on a line perpendicular to the centre of the line connecting the two trees concerned.

If one wishes one can, with a special attitude, interest oneself for and experience surrounding objects' mutual "recedence in the field of vision." If one looks at a photograph or a perspective drawing of the same scene, one immediately considers that it resembles this scene. It lies near to hand to presume that the experienced resemblance is due, among other things, to the relations between the objects' "recedence in the field of vision" being roughly speaking the same as the relations between their "pictured distances" on the picture or drawing. The objects' "pictured distances" are governed by simple geometric laws included under the laws of perspective. It will be demonstrated that objects' "recedence in the field of vision" does not follow the usual geometric laws. In as far as this is the case, this problem is therefore part of the same general field as Rubin's proof that sensorily perceived figures could be non-Euclidian.<sup>1</sup> The matter, however, is more serious than that. It has been shown that such "recedence in the field of vision" does not allow of treatment within the boundaries of normal logic. Due to the fact that objects' "pictured distances" in a perspective picture, in spite of the agreement with their "recedence in the field of vision" which gives rise to the appearance of similarity mentioned above, have been shown to differ from this recedence in

1. E. Rubin: Visual figures apparently incompatible with geometry. *Acta psychol.* 1950, 7, 365-387.

important ways, the latter will therefore be called their "perspectoid distance".

These perspectoid distances have been investigated in the following way. On a table, vertical rods were arranged so that a subject could observe them through a frame D (compare figure 1) on a plain background.

The subject's head was held in a constant position by means of a chin rest. The first experiment involved three rods, which will be called rods 1, 2 and 3. Rod 1 was placed directly in front of the subject's nose at a distance of from 70 to 100 cm. and rod 2 somewhat to the left of rod 1 (10 to 17 cm.) in the same frontal plane as rod 1. Rod 3 could be moved in a frontal plane somewhat behind rods 1 and 2 (about 30 cm.). The positions of the three rods was as shown in figure 1 which gives their position as seen from above.

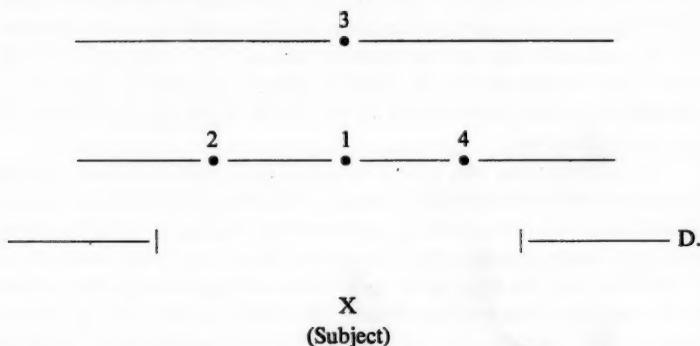


Figure 1.

At the beginning rod 3 was so placed that it was seen by the subject to be behind rod 1. The perspectoid distance between rod 3 and rod 1 was thus 0. If rod 3 was now moved outwards in its frontal plane, the subject saw the perspectoid distance between rod 3 and rod 1 increase, his task now being to inform the experimenter when he considered the perspectoid distance to be as great as the distance between rod 1 and rod 2. The majority of the subjects accepted the task after some time and became very painstaking with their judgments.

The subject was then required to draw what he had seen on a piece of paper of the same shape as the opening in the frame. In nearly all cases he drew three vertical, equidistant lines as in figure 2.

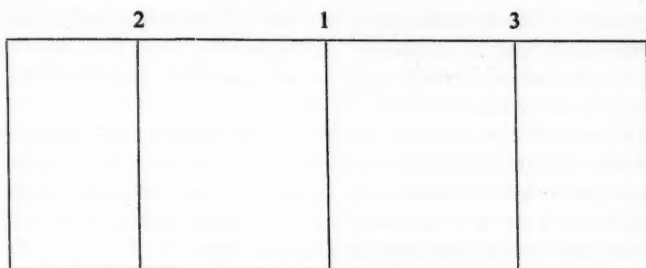


Figure 2.

Rod 4 was now introduced into the experimental situation in such a way that it was moving in the same frontal plane as rods 1 and 2. The subject was now to inform the experimenter when he considered that the distance between rods 4 and 1 was the same as that between rods 1 and 2.

When this had been done the subject was asked to draw into the diagram previously made by him the position in which he saw rod 4. In the majority of cases a fourth line was drawn so that it lay to the right of the lines representing rods 1 and 3, this being an expression of rod 4 being seen as far outside rod 3 (compare figure 3).

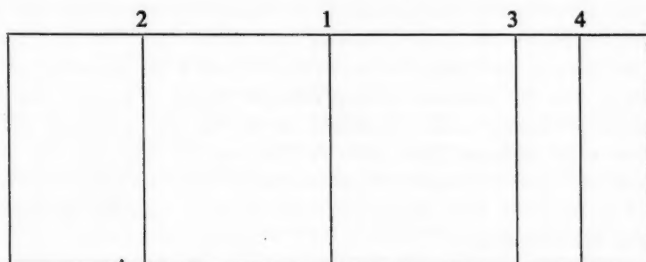


Figure 3.

As a rule rod 4 was first drawn in after some hesitation and surprise about the fact that the picture of rod 4 did not correspond to the picture of rod 3, as the subjects thought that the two distances should both correspond to that between rods 1 and 2 and thus should be the same as each other. Strongly emotional outbursts could be occasioned such as: "What the devil is this?", "There's some crafty work going on here!", "What a sell!", "There must be something the matter with my distance

judgment!", "This is confusing!", etc. One of the subjects took the situation calmly only to accost the experimenter in the street some days later to ask what the mystery was as he had come to the result that "Either you or I must be crazy!".

The peculiar aspects of the experiment are perhaps seen most clearly when one tries to describe it in a logical way as follows: Let us call the experienced distance between rods 1 and 2 'a', the perspectoid distance between rods 1 and 3 'b' and the perceived distance between rods 1 and 4 'c'. As a result of the first judgment made we have:

$$a = b$$

and from the second judgement:

$$a = c$$

but by comparing b and c we find that b is much less than c:

$$b < c$$

and thus not, as one would find if the transitivity of the relation of equality was applicable, that  $b = c$ . It should also be noted here that the whole situation can be perceived at once, and thus the three relationships  $a = b$ ,  $a = c$ ,  $b < c$  do not necessarily have to be perceived successively but can be perceived simultaneously. It becomes even worse if one describes the phenomena in the following way: If one says that the distance a is the same as the distance b and as the distance c (in the same way as one says that the distance between different points is 4 cm.) then the principle of identity itself is exploded, as the distance a is such that it appears to be different from itself. A third way of discussing the phenomena also gives us trouble. If the perspectoid distance between rods 3 and 4 is called 'd' then this is perceived as being considerably greater than 0. We then have:

$$b + d = c$$

but as  $b = a$  and  $c = a$  we should find that

$$d = 0$$

The paradoxes shown here actually demonstrate that the phenomena concerned do not allow of treatment or description in usual logical terms, in that our point of view is that of logical positivism, namely, that logic and the various mathematical disciplines are to be regarded as a type of language (or models) for use in descriptions of observed fields of phe-



nomena. In general one often tacitly presumes that such languages have a far greater span than is actually the case (compare the emotional outbursts of the subjects).

In an earlier experiment the subjects were not required to draw what they saw but should only first adjust rod 3 and then rod 4 in the way described, finding on completion nothing remarkable in the situation resulting. First when they were provoked to a more detailed description of what they saw and when they, as in the majority of cases, chose the usual geometric-logical language, did they notice anything surprising (here one can compare the above mentioned paper by Rubin on visual figures incompatible with Euclidian geometry).

We remarked before that the whole group of rods could be seen at once and in one visual act, and that the three relationships  $a = b$ ,  $a = c$  and  $b < c$  could be present simultaneously in consciousness in spite of the adjustments having been successive. To this, however, must be added that some subjects, after having made the last adjustment, expressed a desire to alter the adjustment previously made as a direct result of everything not agreeing afterwards with the simultaneous perception of the complete arrangement in one visual act which they were asked to try. The change which was usually demanded was that of moving rod 3 a little to the right by which means the difference between  $b$  and  $c$  was reduced. Even though the paradox is not removed by this method it is nevertheless possible to say that the change tends to reduce it. This would appear to mean that when perspectoid distances are perceived *simultaneously* the processing activities tend to make the system *approach* as far as possible a situation where the relation of equality between such distances is transitory. It is not completely successful, and it may be asked what forces are here in conflict. In this respect the following considerations can perhaps be of value.

The paradox concerning perspectoid distances is related to the problem of constancy of form. If one is sitting at a dinner table, one's plate appears to be perfectly round in spite of the fact that one does not observe it from a point vertically above its centre. In our language one can say that the perspectoid distances between each set of diametrically opposite points on the circumference of the plates are equal. It is a matter of taste whether one will say that our phenomenon concerning perspectoid distances is a special case of form constancy or an elementary phenomenon which can explain form constancy. In any case, the fact that the perspectoid distance  $b$  is subtended by a far smaller visual angle than the equally large perspectoid distance  $a$  is the cause of rod 3 being seen

as lying far within rod 4. *One can thus say that a constancy phenomenon is in conflict with the demand for transitivity of the relation of equality, and that it must be presumed that the result always must be a product of a functional compromise.*

For sake of completeness it is noted that, without being able to go further into the functional genesis of it here, experiments have been made with "solid" distances (rectangular cardboard sheets, the vertical contours of which substituted for the rods in the above experiments). The experiments showed that "solid" distances tend towards increasing the strength of the paradox.

Corresponding to perspectoid distances I have also worked with perspectoid directions and sizes in the field of vision, and have there found similar paradoxical conditions. It can also be pointed out that these perspectoid phenomena play a large part in the establishment of our visual space. They form a peculiar closed system possessed of its own laws.

# EXPERIMENTAL HOMOSEXUAL BEHAVIOR IN MALE ALBINO RATS

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## CONTENTS

1. Statement of the problem . . . . .	303
2. Apparatus . . . . .	304
3. Procedure . . . . .	304
4. Results . . . . .	307
5. A comparison between the different test series . . . .	324
6. Discussion of results and procedures . . . . .	325
Summary . . . . .	331
Zusammenfassung . . . . .	332
References . . . . .	333

## 1. STATEMENT OF THE PROBLEM

Opinion is divided as to the causes of homosexuality in human beings. Heredity plays an important part according to some. Others emphasize the fact that isolation from the opposite sex provokes the tendency. Other investigators lay stress on internal secretion, purely organic reasons, and so on. One theory is that punishment, prohibition, social disapproval etc. gradually and slowly foster an inhibition towards the opposite sex, then this inhibition will finally bring about the result that the individual's sexual desire will seek an outlet with individuals of the same sex, even when they apparently have every chance of finding immediate contact with the opposite sex.

It is quite impossible to have any decided opinion about the relative importance of the above-mentioned causes, as long as there are so few experimental data and reliable investigations to work on. Because of these circumstances we have tried to transfer the problem to experiments with animals, in order to see how far the last mentioned theory might have a biological foundation. We have therefore given a male a slight electric shock in a special apparatus every time it tried to mate with a female, in order to see to what extent it would try to mate with another male when the inhibition towards females was developed.

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The experiments were performed some years ago (1941-1942), but have not been published before first because of the war, and later because of other circumstances. This delay in publication of the results has proved to be advantageous as we in the meantime have conducted other sexual-biological investigations which help to understand the problem.

## 2. APPARATUS

The apparatus used in the investigations is reproduced in Figure 1.

The source of current is in a laboratory next door. The shock given to the males in all these series of tests has been 500 volts (alternating current, 50 cycles) and 1 milliamper. The electric arrangement for producing this stimulus has been described previously (11). The coupling schedule of the grill which forms the bottom of the apparatus is given elsewhere (12).

We have used a resistance of 500,000 ohms. Measurements taken in this laboratory have shown that the rat's resistance is in the neighbourhood of 150,000 ohms (16).

It seems that the humidity of the air has something to do with the magnitude of the resistance which is mainly a transitional one (between the wires and the feet of the rat). If the degree of humidity of the air is high, as for instance in summer, the resistance is much lower, about 50-60,000 ohms (16). We cannot say, however, anything definite about it, and systematic measurements under varying humidity of the air must be carried out.

Starting from the standard deviations for the measurements taken in the different seasons, we can calculate that  $\frac{2}{3}$  of the shocks the rats have received in all the following series of tests lie within a variation of 0.05 milliamper. As the resistance was shown (16) to vary as much from shock to shock for each rat as from rat to rat in the same series of measurements, there should be no reason to suppose that these variations play any important part for the results obtained in the individual series of experiments.

Quite another matter is the fact that the four different series of tests were made in different seasons, so that we may suppose that there is a slight difference in the average strength of the shocks the rats received from the one series of tests to the other. This does not, however, play any role for these investigations, as the different series of tests are not *directly* comparable in any case.

The shock the rats have received has in all cases lasted for 200/1000 seconds. The mechanism which gives this shock is composed of a compound-shutter from a camera which is electrically coupled. A drawing and description of this are given elsewhere (12).

## 3. PROCEDURE

The animals used have been on a somewhat modified McCollum's diet. The living room of the rats has warm ventilation with automatic regulation of the temperature which is kept at 19-20° C. In the whole series of tests, females between 9-13 months old have been used. It is practical to use older females as stimulus animals, as young ones will as a rule react against the rubber socks they must have on their feet (see later).

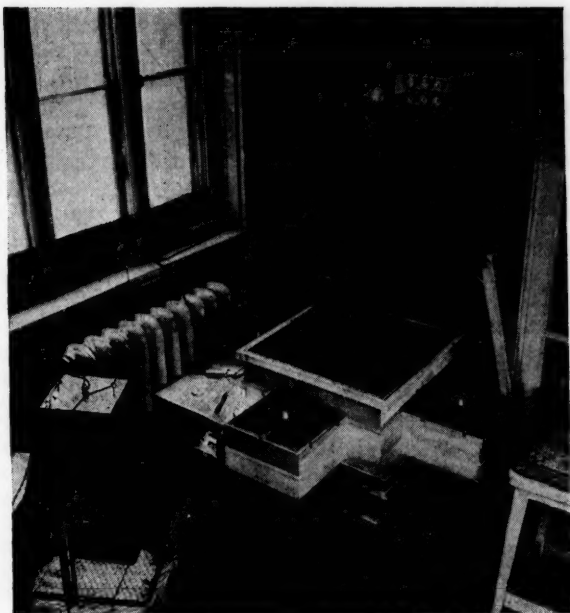


Fig. 1. The Shock Apparatus.

The apparatus consists of a box  $50 \times 50 \times 25$  cm standing on four legs 10 cm high. At the sides of the box will be seen smaller compartments. These are separated from the box and are not used in this investigation.

The entire floor of the apparatus is made up of 1 mm brass-wires, 67 in number. The wires are fastened to a frame and alternately connected with one of two main wires on opposite sides of the frame (11). When the current is closed, a rat in the box will receive an electric shock.

Underneath the box there is a tray with sawdust, and over it a wire netting cover, which may be removed. Note the illuminating box over the cover, and likewise the mirror which will facilitate observation.

A slab of wood is fastened to the left lower side of the apparatus on which the journal rests, being illuminated by a weak lamp. Underneath the slab is the amperimeter and the apparatus which closes the current for 200/1000 seconds. A drawing of this apparatus may be found elsewhere (12).

In the foreground we see a cage with a lighting box above it. When not in the shock apparatus the female with rubber socks is placed in this cage which can be reached from the chair (not seen in the photo) on which the observer sits.

In the background we observe Hipp's chronoscope for controlling the duration of the shock about twice a week. On the wall above the apparatus are a hygrometer, a thermometer and the thermostatic installation, which keep the temperature of the laboratory constant at  $10^{\circ}$  C. The radiator seen in the immediate neighbourhood of the apparatus is put out of function.

Females to be employed in copulation tests are picked out by stroking a number of females in a cage on their hind parts with the hand: the animals in the sexual phase, which are willing to mate with males, show a very typical behavior on this treatment. The sexual behavior of female rats has been described by Ball (1), Hemmingsen (8), Beach (2, 4, 5), Rasmussen (14, 17) and others. Photos of the different characteristic positions of the sexual patterns of behavior are also to be found in the literature (7, 8).

In order that the females do not also receive the shock when the current is closed in the grill, their feet have to be isolated. Finger-stalls of thin rubber are used for this purpose.

For the hind feet, the finger-stall is rolled out about 4 cm, and turned inside out. While the rat is held in the left hand, the finger-stall is put on with the right. A pair of pincers with a rubber band lie ready on the table and are also taken up in the right hand. The metal bars (see later) over which the elastic band is stretched are opened, drawn over the rat's leg, and the elastic band is then transferred from the pincer to the leg. When the band is in position round the back joint between the leg and foot (the *articulatio talo-cruralis*), the finger-stall is rolled back over the band as far down over the foot as it can go.

For the foreleg the finger-stall is rolled out about 2.5 cm. In this case the finger-stall is *not* turned inside out, so that the rat may not have any difficulty in drawing its forefeet back if it should chance to put them down between the wires. After the elastic band is securely in position, the finger-stall is drawn back a little over it. The band must not be too tight round the leg, as it might impede the blood circulation and bother the female so much that she will refuse to mate. Neither must it be too loose, so that the rat can draw it off. We have used elastic  $1.5 \times 1.5$  mm. Of this we have made small bands of suitable size by knotting. Practice gives experience here.

In order to get the bands onto the legs of the females we have soldered small metal bars to a slide pincer. The bars go out to the side a little so that the elastic bands can be stretched over them. Figure 2 shows a drawing of this special pincer and a description will be found under the drawing.

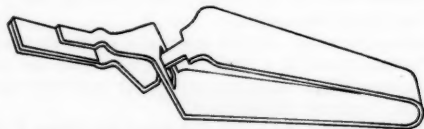


Fig. 2. The pincers with which we put the elastic bands round the female rat's leg. They are 12 cm long. The metal bars over which the band is stretched are 19 mm long and soldered to the slide pincer. They protrude slightly (about 3 mm) beyond the beak of the pincers, enough to give the band room to sit securely.

When the pincers are pressed together, the bars on which the band is stretched go farther apart, and can be drawn up over the leg of the rat. The elastic band is then transferred from the pincers to the leg.

The process of supplying the females with such rubber socks requires a certain amount of practice if it is to be done properly. And some bites may be expected (it is impractical to use gloves) before the necessary sleight of hand is acquired. The process is described in such detail because the rats must naturally be handled in such a way that they will be willing to mate after they have their rubber socks on.

The male rats must be given an opportunity to get accustomed to the apparatus, otherwise they may not mate when the test begins. They are placed in the apparatus at 6-7 A. M., after the night's work is done, and have been in it all day and evening when the tests begin at 11-12 P.M. Food and water are of course available in the apparatus all the time. Rats are very sensitive to the deprivation of water and food and loose weight rapidly (11, 12, 13, 16).

The rats used in the tests have had plenty opportunity to mate with females from the age of 3-4 months until they are tested. It is a fixed rule in this laboratory that females (about 30) are put in with the males to be used in copulation tests on the 23rd of each month. The animals are then together until the 12th or 13th of the following month, when all females are removed. New females are then put in to the males on the 23rd, and so on. The males have thus been isolated for about 10 days in each month, and the majority of the females are impregnated in the course of the first half of the breeding period, so the males have not always had full access to females willing to copulate. None of the males used in the tests has been isolated from females for less than 4 days, as males which have recently been together with females in phase may sometimes prove to be not very potent. Details of the procedure vary for the different series of tests and will be described as we go along.

#### 4. RESULTS

##### *a) The first series of tests.*

The first series of tests was made during the period April 17th to May 1st, 1940, from 12 P.M. to 8 A.M. The males used were 11-12 months old. Of the original group of animals 8 would not mate and so could not be used for the tests. Fifteen males were test animals and 15 were controls, the original homogenous group thus comprised 38 animals. We must always make allowance for the fact that a larger percentage of males will not mate other than under usual conditions in the home cage, either because they are unaccustomed to the wires or because the female has rubber socks on. The procedure was as follows: 2 males in the apparatus for 2 min. (to see whether the animals showed homosexual behavior before

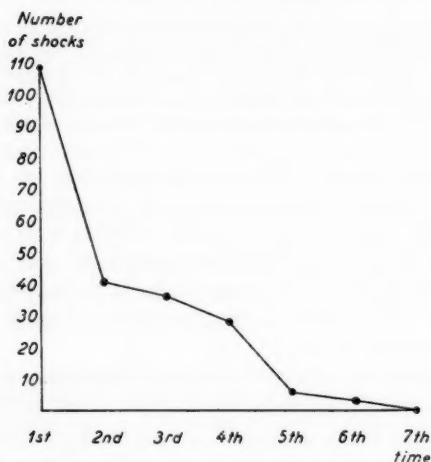


Fig. 3. First series of tests. Total number of shocks for 15 test animals during the 7 different 10 minute observation periods.

the test). Then the one male was removed and a female put in. The animals then had 2 copulations with these females. The mean lapse of time until the first coitus was 28 seconds, until the second coitus 68 seconds.

Subsequently, the male was given a shock each time it tried to mate with the female. The shock was given exactly at the moment when the male had placed its forelegs over the female (having rubber socks on her feet) and was about to begin coitus movements. Period of observation 10 min., then an hour's interval (counted from the start of the test) until the next test. After the male had been 1 min. alone in the apparatus in order to get accustomed to it, a female was put in and a shock given each time the male tried to mate. 10 min. observation period, 1 hour's interval. This was continued until the inhibition towards the female was firmly established. As proof that the inhibition was established, we used the criterion that the male did not try to mate with the female for the space of 2 min. When this result was achieved, another male was put in, and the number of attempts at coitus the test male had with the other male and contingently with the female in the course of 3 min. was recorded.

Figure 3 gives the total number of shocks for the animals tested.

We see that there is a marked fall in the total number of shocks from the first to the second time, and that afterwards, the curve falls steadily until the 7th time when the inhibition is established for all the animals (no animal has tried to mate with the female for 2 min.).



The animals received almost half or, to be exact 48.5 per cent of the total number of shocks (225) in the course of the first observation period. Moreover, the great majority (58 %) of shocks were given during the first 2 min. every time the animals were tested.

In the second column of Table 1 is the number of shocks necessary for each of the 15 test animals to prevent them from copulating with the female.

Table 1. First series of tests. The 4th column gives the number of attempts at coitus with another male during a 3 min. period of observation after the "inhibition" with regard to the female was established.

Male no.	Shocks given	Copulations with female	Attempts at coitus with male
329	31	2	9
338	35	0	1
343	19	1	5
347	10	4	3
360	30	0	4
367	4	3	3
369	5	0	4
373	7	0	0
376	6	2	0
378	3	0	2
380	4	0	1
382	23	4	5
384	18	4	0
386	20	0	4
389	10	0	8
Total	225	20	49

There were great individual differences in the number of shocks. Two of the males did not try to mate with the female when placed in the apparatus again after having received shocks during the first observation period. The inhibition is progressively established in more animals, and the 6th time only 2 tried to copulate with the female.

The number of attempts at coitus with the other male and number of copulations with the female after the inhibition with regard to the latter was established will be evident from the last two columns of Table 1.

From the left part of Table 3 we see that the males had altogether 18 attempts at coitus with the introduced male in the course of the first 30 seconds - whereas only 1 copulation with the female was executed. The male which performed this one copulation with the female, male no. 347, was one of the seven males which attempted to copulate with the male in

the first 30 seconds. From our journal it is evident that this male first had 2 attempts at coitus with the male and then the 1 copulation with the female. The male had 2 more copulations with the female, then 1 attempt at coitus with the male (in the period from 1½–2 min.), and finally 1 copulation with the female in the last 30 seconds of the observation.

From our journal it is seen that the male achieved ejaculation at this copulation. None of the other test males achieved ejaculation on copulation with the female.

In the table we see that the test males had in all 20 copulations with the female. As many as 7 of them mated finally with the female. This fact is partly due to "social facilitation": the other male which was intended to play a rather passive role in this "experimental triangle" used the opportunity to mate with the female. On noting this the test male grew more "courageous". We have elsewhere (11) experimentally demonstrated that rats in groups are more "courageous" than a single rat when they try to drink from a cup of water where they receive an electric shock in our apparatus after having been deprived of water for a time.

Turning to the last column of Table 1 we note that 12 of the 15 males tried to perform the act of coition with the other male. Three of these 12 had, however, less than 3 attempts, and only a few attempts at coitus with another male are most correctly to be regarded as "mistakes". Two of the remaining males had 3 attempts, which perhaps also is rather a small number. But if we say that *about half of the animals showed behavior which was of a definitely homosexual character*, we should be quite sure that we have not exaggerated.

The first animal presented in the table, male no. 329, first performed 7 attempts at coitus with the male, then 2 copulations with the female (in the period from 1 to 1½ min.) and finally 2 more attempts at coitus with the male. At the last of these attempts, the male *achieved ejaculation*. The ejaculate, coagulating when leaving the penis, attached itself to the hair of the other male's back where it remained for some time until pulled off by the observer.

A closer study of the table makes clear that 7 males (nos. 338, 360, 369, 378, 380, 386 and 389) showed exclusively homosexual behavior. 2 males (nos. 376 and 384) displayed exclusively heterosexual behavior. Disregarding male no. 373 which showed no sexual activity at all, the remaining 5 males displayed both homo- and heterosexual behavior.

A comparison of the different parts of the table shows that the animals which had many attempts at coitus with the male generally speaking also had to be given many shocks before the inhibition with regard to the female

was established. As the group does not comprise more than 15 animals, we do not feel inclined to give a correlation coefficient. But if we in one group take the animals which have from 9 to 4 attempts at coitus with the male (the males nos. 329, 343, 360, 369, 382, 386 and 389), it can be calculated that they received an average of 19.7 shocks – whereas the remaining males which have from 3 to 0 attempts at coitus with the male, received 10.9 shocks.

But there are exceptions. Male no. 369 which had 4 attempts at coitus with the male had to be given only 5 shocks, while male no. 338 which had only 1 attempt at coitus with the male had to be given 35 shocks.

The number of shocks an animal must be given in order to keep away from a stimulus may be used as a measure of the strength of the need in question. We have thus elsewhere (12) used the shock apparatus for measuring the strength of the hunger drive after different intervals of food deprivation.

If it can be confirmed with a larger number of animals that those which have the highest heterosexual drive (receive the greatest number of shocks) also will have high ratings for homosexual behavior when an inhibition toward females is established, it will be of great significance. As the inhibition toward females has not been fully established in the following series of tests, this investigation will not give further information on this important point.

The procedure for the control animals was exactly the same, except that for them no inhibition in regard to females was established. A control male was observed immediately after the first observation period for each test animal. The mean lapse of time until the first copulation was 35 seconds, until the second copulation 101 seconds. Then the control males were put in separate cages. As soon as the test animal had finished, the control animal was tested, and the number of copulations it had with the female in the first 2 min. was recorded, then the number it had with the female and contingently with the male in the next 3 minutes. Table 2 shows the results for these 3 min.

It will be seen that the control animals had 36 copulations with the female. The fact that they did not have a larger number of copulations is to be explained by the circumstance that they mated during the 2 min. of observation – during which time the test animals had no coitus. During this period, the control animals mated 63 times. If we take the whole time of observation (2 min. with a female only, 3 min. with 1 male and 1 female), then we get the result that the test animals mated 20 times with the female (Table 1) while the control animals mated 99 times during these five minutes.

Table 2. First series of tests. The table presents the results for 15 control animals.

Male no.	Copulations with female	Attempts at coitus with male
333	2	0
340	7	1
344	0	0
349	1	0
361	4	0
365	1	0
372	0	0
374	0	0
377	5	1
379	2	0
381	3	0
383	4	0
385	3	0
388	1	0
393	3	0
Total	36	2

Table 3. First series of tests. The table gives the distribution of the number of copulations with the female and attempts at coitus with another male during the 3 min. period of observation.

Parts of observation period in min.	15 test animals		15 control animals	
	Copulations with female	Attempts at coitus with male	Copulations with female	Attempts at coitus with male
0- .5	1	18	9	2
.51-1.0	6	14	6	6
1.01-1.5	5	8	4	0
1.51-2.0	4	3	5	0
2.01-2.5	1	2	5	0
2.51-3.0	3	4	7	0
Total	20	49	36	2

We observe further in Table 2 that 3 of the control males did not copulate with the female at all. Nor did 2 of these, nos. 344 and 374, mate during the 2 min. period of observation (with the female alone). It is rarely seen that a male which is removed after a few successful copulations will not mate when put to females again the same night. The fact must probably be ascribed to the unusual conditions: the wires and the rubber socks of the female.

The third animal which did not copulate, male no. 372, copulated 8 times during the 2 min. observation period and achieved ejaculation. The 3 min. period of observation with a female and a male (Table 2) thus coincides with the post-ejaculatory quiescent period (Stone, 20).

Such a coincidence has also to a lesser or greater extent occurred for many of the other animals. Two more males, nos. 365 and 383, also obtained ejaculation in the course of the 2 min. observation period. As seen in Table 2 they began to copulate again after the post-ejaculatory quiescent period before the close of the 3 min. observation period.

During the 3 min. observation period (with the female and the male), as many as 9 males achieved ejaculation (nos. 333, 340, 349, 361, 377, 379, 385, 388 and 393). The males which achieved ejaculation performed no more copulations during the observation. The post-ejaculatory quiescent period usually lasts about 5 to 10 minutes (14, 17, 21).

None of the males achieved more than 1 ejaculation during the 5 minutes, nor could this be expected. In a group of 120 copulating males the shortest time elapsing from the beginning of the observation period until the second ejaculation was shown to be 7 min. and 19 sec. (14). It is easy to decide when a male achieves ejaculation. The 3 criteria for this are described elsewhere (17).

The control animals had 2 attempts at coitus with the male. As previously mentioned only a few attempts at coitus with a male are most correctly to be regarded as "mistakes". When a male is together with a receptive female, it is often so eager that it has no time to explore whether the object of its attentions is a male or a female. But the male is at once abandoned.

None of the males, whether test or control, showed any homosexual behavior *before* the test.

*b) The second series of tests.*

This series of tests was made during the period August 22nd to 30th, 1940, from 12.45 A.M. to 7 A.M. Males 240-250 days old were used. As many as 9 of the animals which were to be tested refused to mate, so this series of tests comprises only 12 test animals. In addition to those previously given, the reason why so many animals refused to mate may possibly be the fact that these animals in their youth had been used for some hunger tests and had thus not received sufficient food while they were growing up. The animals which were unwilling to mate were rather lighter in weight (mean 295 g) than the potent animals (mean 336 g). While no general correlation seems to exist between body weight and number of

copulations during a fixed observation period, impotent animals might generally have a lighter body weight than potent ones (14, 17).

The procedure for this series of tests was: first, 2 males alone in the apparatus. None of the animals showed any homosexual behavior. At the end of these 2 min., a female was put in. When the test animal had had 2 copulations with the female (mean lapse of time until the first coitus 81 seconds, until the second 150 seconds), the other male was removed, and the test animal was given a shock every time it tried to mate with the female. Period of observation 8 minutes. Then the other male was put into the apparatus and the number of attempts at coitus with this male and of copulations with the female were recorded during a 2 min. period of observation. Then all three animals were removed from the apparatus. The test male has of course in all series of tests been put in a separate cage.

After an interval of 30 min. (from the beginning of the test), the test male and a female were replaced in the apparatus and the male given a shock each time he tried to copulate. Then a male was introduced, and the test animal allowed to copulate *ad libitum* with this or the female for 2 min., then 30 min. interval, and so on. This procedure was repeated until the animals had been 6 times in all in the apparatus.

The second column of Table 4 shows the number of shocks given to the animals. Table 5 gives the distribution of the number of shocks in the observation periods. The fact that there are so many shocks for the last tests in this series as compared with the first test series (Figure 3), is due to the one coitus the test animals have had in some cases without having received a shock. This corresponds fully to what we know about conditioned reflexes: when the conditioned stimulus is absent, the reaction is weakened.

Disregarding the exceptional rating of 16 shocks for male no. 473 the first time, we may say that the animals received about the same number of shocks in the last tests as in the first. The positive urge to copulate with the female is about equally as strong as the avoiding reaction produced by the shocks throughout all the periods.

This fact will also be evident from the 3rd column of the table showing that the males accomplished about equally as many successful copulations with the female during the last tests as during the first when observed for 2 min. after each shock period.

From Table 4 it is further evident that as many as 9 of the 12 males performed one or more successful copulations with the female. It is clear without further comment that the achievement of these copulations has rendered it more difficult to establish the inhibition.

Table 4. Second series of tests. The table gives the individual results for 12 test males.

Male no.	Shocks given	Copulations with female	Attempts at coitus with male	Ejaculations
465	20	2	31	0
466	19	0	22	0
467	43	6	0	1
468	22	1	29	0
471	22	12	8	2
473	53	5	1	1
774	20	1	28	0
475	17	2	3	0
476	16	0	30	1*
477	28	4	0	0
482	11	0	49	0
485	14	4	4	0
Total	285	37	201	5

\*with male

Table 5. Second series of tests. Total results for 12 test males.

Observation period	Shocks given	Copulations with female	Attempts at coitus with male
1	63*	6	21
2	48	4	44
3	42	5	38
4	33	10	38
5	57	7	34
6	42	5	26
Total	285	37	201

\*No. 473 had to be given 16 shocks.

It will be seen in Table 4 that 3 males also achieved ejaculation with females. In the one male (no. 467) the ejaculation was elicited after only 1 copulation, in male no. 471 after 3 copulations, and in no. 473 after 2 copulations (in the same period of observation). The whole procedure must have provoked a very high charge of the sex drive (17) in the animals. It is remarkably exceptional that ejaculation is achieved after so few copulations. Usually about 7-10 copulations are necessary to elicit the first ejaculation during an observation period (14, 17, 4). The second and following ejaculations are achieved after a lesser number (about 4-5) copulations (14, 17).

The majority of shocks was given during the first 2 minutes of each observation period.

The number of attempts at coitus with another male will be evident from the fourth column of Table 4.

The individual differences were great. The animals fall into two distinct groups: either they have no or few attempts at coitus with the male or they have many. As there was an equal number of animals in each of these two groups, we may say that also in this series of tests *about half of the animals showed a behavior definitely homosexual in character after having received a shock each time they tried to copulate with a female.*

As seen in Table 4 one of the males achieved ejaculation with another male. The ejaculate adhered for a short while to the hair of the other male's back, but soon dropped off by itself.

According to the number of shocks, the animals may be divided into 2 groups: in the one we have those which received the highest number of shocks (nos. 473, 467, 477, 471, 468 and 465), in the other group those which received relatively few shocks (nos. 474, 466, 475, 476, 485 and 482). The males nos. 465 and 474, which both have 20 shocks, have, as seen in Table 4, about equal ratings in the other functions. If they are exchanged, it will have practically no influence on the final results of the two groups.

The present grouping for the above two animals is unfavourable to the phenomenon which we wish to demonstrate: homosexual behavior is less readily provoked in animals with a high sex drive (which receive many shocks) than in animals with a low.

The 4 males receiving the highest number of shocks clearly demonstrate this. During the 8 minutes observation they repeatedly try to copulate, are each time frightened away from the female by the shock, but after the close of 8 minutes they still mount the female and finally succeed in copulation. As seen in the table, 3 of these 4 animals were the only ones to achieve ejaculation at a copulation with a female. *They successfully defy the inhibition with the procedure used in this series of tests, their sex drive does not need to seek relaxation (17) through homosexual behavior.*

In 6 of the remaining 8 animals, a behavior definitely homosexual in character is provoked (from 49-22 attempts at coitus with another male). Male 485 showed no homosexual behavior and 475 had, as seen in the table, only 3 attempts at coitus with another male. These 3 attempts are of no significance. These two animals had on the other hand some copulations with the female.

The animals which showed *exclusively homosexual* behavior, males nos. 466, 476 and 482, are all to be found among the males receiving the lowest



number of shocks – whereas 2 of the 3 males (nos. 467, 477 and 485) displaying *exclusively heterosexual* behavior are found among the males with many shocks and 1 among the males with few shocks.

The remaining half of the animals (nos. 465, 468, 471, 473, 474 and 475) showed partly homo- partly heterosexual behavior. Among these males nos. 465, 468 and 474 displayed preponderantly homosexual behavior, having, as seen in the table, only 2, 1 and 1 copulations respectively with the female. Male no. 473 showed preponderantly heterosexual behavior, having only 1 attempt at coitus with the male during the first period of observation and thereafter 5 successful copulations with the female. Finally males nos. 471 and 475 had about equal ratings for homo- and heterosexual behavior.

From our journal it is evident that the males which displayed both homo- and heterosexual behavior during the *same* observation period, always showed the homosexual behavior first and then the heterosexual. If the inhibition towards the female is once overcome, the male, having had one copulation with the female, *never* returns to homosexual behavior in the same observation period. Quite another matter is the fact that the male might attempt to copulate with the male again during the next observation period after the inhibition towards the female has been built up more thoroughly by more shocks.

Note that in this series of tests there are relatively fewer animals showing exclusively homosexual behavior as compared with the first series of tests (Table 1) in which the inhibition toward the female was more firmly established.

The great majority of attempts at coitus with another male in all 6 observation periods was performed in the course of the first half of the observation period, and especially during the first 30 seconds (47 %).

### c) *Third series of tests.*

This series of tests was carried out during the period October 25th to November 16th, 1941, from 11 P.M. to 5 A.M. with males 252-263 days old. Seven of the original group of 22 animals would not mate. The procedure was: 2 males in the apparatus for 2 minutes. Three of the animals, nos. 495, 499 and 511, showed homosexual tendencies (NB *before* any inhibition was established against the female). But as it is a question of only a few attempts at coitus with the other male, it is very doubtful whether any emphasis should be placed on this phenomenon. Then a female was put in and the test animal had 2 copulations with it. The mean time for the first copulation was 54 seconds, mean lapse of time until the second cop-

Table 6. The results obtained in the 3rd and 4th series of tests.

Observation period	3rd series of tests		4th series of tests	
	Shocks given	Attempts at coitus with male	Shocks given	Attempts at coitus with male
1	85	83	96	9
2	45	53	58	40
3	22	44	35	28
4	13	50	32	52
5	13	42	33	44
6	11		33	45
Total	189	272	287	218

ulation 130 seconds. After the test animal had had these 2 copulations, the other male was taken away, and a shock was given every time the test male tried to mate. Period of observation 5 minutes. Then the animals were taken out of the apparatus. There was an interval of 30 minutes (from the beginning of the test) until the next test. After the test male had had 1 minute to get accustomed to the apparatus, another male was introduced. Period of observation 2 minutes. The other male was taken away, the female was put in, and the work of establishing the inhibition was continued until the animals had been 6 times in the apparatus.

Note the difference in procedure as compared with the former test series. Here the two males are together *before* each shock period, while in the former test series, they were together immediately after. Moreover, in this series of tests the test male is never together with both the female and male at the same time after having received shocks.

Disregarding the two first activation copulations, the test animal is given no opportunity to accomplish the act of coition with the female; it always receives a shock when trying to do so. Also on this point the procedure differs from those employed in the first and second series of tests.

Table 6 gives the number of shocks in the second column.

It will be seen that the number of shocks, as in the 1st series, first diminishes rapidly, then more slowly. The fact that they received relatively more shocks during the 5th and 6th periods than the animals partaking in the first series (cf. Figure 3) may perhaps be ascribed to their having been allowed to mount a male during the first part of the test (in contrast to the first series) without receiving a shock.

But these attempts at coitus with a male have evidently not been so effective as occasional successful copulation with a female in stimulating

the *libido sexualis*: each of the last 3 times they received only about one third of the total number of shocks administered to the males in the second series of tests (Table 5) in spite of the fact that this group comprises 3 more animals.

The number of attempts at coitus with another male will be evident from the third column in Table 6.

Remember that the attempts at coitus in the first observation period are performed just before the animals receive shocks during the second period, the figure 53 is the number of attempts at coitus with male just before the third shock period, etc.

The phenomenon evident in the second series of tests also appears here:

The figures fall into two rather distinct groups: either the male tries to copulate many times or he has no, or only a few attempts at coitus with the other male. As 7 males tried to copulate more than 20 times, we state that here also *about half of the animals showed behavior definitely homosexual in character.*

None of the males achieved ejaculation on coitus with the other male. They were, to be sure, not subjected to the exciting influence which the presence of a receptive female must be supposed to be, even if they do not venture to mount her.

As in the second series of tests the great majority of attempts at coitus with another male in all 5 periods of the observation was performed in the beginning of the observation period (45 % during the first 30 seconds).

In order to make clear the relation between number of shocks and number of attempts at coitus with another male, we present a new table, Table 7.

As we see the results of the animals are set up in two columns according to the number of shocks. Male no. 513, having the median position in the rank order for number of shocks, is kept separate in order to have the same number of animals in each of the two groups.

It is clearly seen in the table that the animals which have the fewest number of shocks, have a considerably higher number of attempts at coitus with another male than those which receive a high number of shocks. There are, however, two exceptions from this rule. Male 511 which has 16 shocks has as many as 38 attempts at coitus with another male - while male 510 receiving only 6 shocks has no attempts at coitus with the male at all.

The results for the remaining 12 animals confirm the results obtained in the second series of tests: some animals will resort to homosexual behavior after a relatively low number of shocks - while others, although they receive repeated shocks, try to mount the female. Offered a male they

Table 7. Third series of tests. The animals receiving many (I) and few shocks (II) are found in separate columns. The number of attempts at coitus with another male are quoted in corresponding columns (I and II).

Male no.	Shocks given		Attempts at coitus with male	
	I	II	I	II
494	15		1	
495	33		7	
496	11		0	
497	23		12	
498	20		3	
499		8		22
501		6		38
506	12		6	
508		6		25
510		6		0
511	16		38	
512		9		55
513		10		7
516		7		36
518		7		22
Total (7)	130	49	67	198
<i>M</i>	18,6	7,0	9,6	28,3
Total (15)	189		272	

will not, as a rule, try to copulate with him – they strive for a heterosexual relaxation of the drive as long as possible.

But when the inhibition has once been firmly established in such animals, they will engage more actively in homosexual behavior than animals with a low sex drive (having received few shocks, first series of tests, Table 1).

*d) Fourth series of tests.*

The tests were made during the period February 20th to March 7th, 1941, from 12 P.M. to 6 A.M. with males 160-180 days old. Only 2 of the 22 animals available would not mate, so here 20 animals were tested. The procedure was: 2 males alone in the apparatus for 2 minutes. Two of the males, nos. 527 and 530, both attempted to mate with the introduced male once. This has no significance as a male ought to be allowed to ascertain by a single mounting whether the object is a female and willing to mate without its being accused of "homosexuality". Then the test animals had, as before, 2 successful copulations with the female. Mean activation time

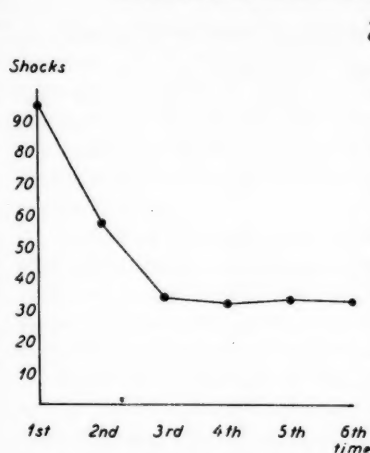


Fig. 4. Fourth series of tests. Total number of shocks for 20 test animals during the 6 different 5 minute observation periods.

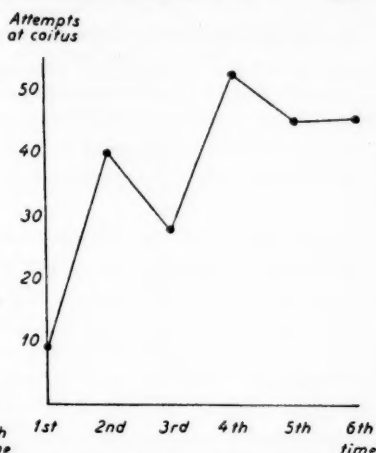


Fig. 5. Fourth series of tests. Total number of attempts at coitus with another male during the 6 different 2 min. observation periods.

for the first coitus was 57 seconds and lapse of time until the second coitus 126 seconds. Then the other male was removed and shocks were given.

After a 5 minute period of observation the other male was put into the apparatus again, and the number of times the test animal tried to mate with it before contingently mating with the female was recorded. If the test male mated with the female before 2 minutes (the fixed observation time) had elapsed, then he was at once removed and the inhibition more strongly established after an interval of 30 minutes (from the beginning of the test). This was continued until the animals had been in the apparatus 6 times. Figure 4 gives the total number of shocks for the 20 test animals.

Comparison with Figure 3 (first series of tests) shows clearly that it is more difficult to establish the inhibition. This is caused, as already mentioned under the second test series, by the one coitus some of the animals have with the female without receiving a shock.

The total number of shocks given during the 6 different observation periods is also evident from the fourth column of Table 6.

It was found that the variation in number of shocks was slight compared with the former test series. Whether this reduced variability is due to the procedure used in this test series or to other factors e.g. greater homo-

Table 8. Fourth series of tests. The table shows the results for 20 test males.

Male no.	Shocks given	Attempts at coitus with male
536	19	1
537	13	36
538	10	32
547	16	0
546	13	24
545	13	0
544	20	4
543	18	0
542	15	0
541	23	7
540	10	0
523	15	19
525	16	28
526	10	2
527	11	6
528	12	20
529	15	24
530	9	7
531	13	0
532	16	8
Total	287	218

geneity of the strength of the sexual drive among these animals, can not be decided on the basis of the data at hand.

The great majority (62 %) of shocks were given during the first minute of the 6 test periods.

Figure 5 gives the total number of attempts at coitus with another male.

We see that the curve is irregular, but on the whole there is a rising tendency. If we compare this curve with the number of shocks (Figure 4), we notice that *as the inhibition toward females is more firmly established (the number of shocks decreases) the homosexual tendency increases.*

The irregularity of the curve in Figure 5 is not surprising if we consider the high variability in number of attempts at coitus with another male as revealed in the third column of Table 8.

The individual variation is great. Only 7 animals can be said to have displayed behavior *unmistakably* homosexual in character (from 36 to 19 attempts) – while 6 males showed no homosexual behavior at all as seen in the table.

One male, no. 525, achieved ejaculation on the back of the other male

after having performed 6 attempts at coitus with this male during the 4th 2 minute period of observation. From our journal it is evident that this male had previously performed 2 copulations with the female, one the first time and one the 3rd time. During the fourth time he had not had any copulation with the female of course as he would then have been immediately removed as mentioned under the description of the procedure for this series of tests.

Turning to the comparison between number of shocks and number of attempts at coitus with the male (Table 8) and noting the relatively low variability in number of shocks, any great difference in the number of attempts at coitus with another male between those males receiving a relatively high and low number of shocks, can hardly be expected.

The 10 first animals in the shock rank order, received a total number of 173 shocks and had a total number of 91 attempts at coitus with the male. The other 10 males, lowest in the rank order, received a total number of 114 shocks and attempted to copulate with the male 127 times. Taking the high variability in table 8 into consideration, this difference of 36 attempts at coitus with another male in favour of those receiving the *lowest* number of shocks, is of very little significance, but must be regarded as a corroboration of the results obtained in the 2nd and 3rd series of tests.

In conformity with the 2nd test series, the animals having the most shocks also generally had more copulations with the female than those receiving a smaller number of shocks. The 10 animals receiving 173 shocks had 20 copulations with the female – compared with 14 copulations accomplished by those having a total number of 114 shocks.

In order to spare the reader for more tables, we mention here in the text that these 34 copulations (c) for the whole group distributed themselves as follows: first test 8 c, 2nd 5 c, 3rd 8 c, 4th 4 c, 5th 5 c and 6th 4 copulations.

As there was an interval of at least 30 minutes between each coitus, none of the males achieved ejaculation. Even if an enforced pause between each coitus is not longer than 10 minutes, the males will have to execute above *twice as many* copulations as when copulating *ad libitum* in order to achieve ejaculation – if they then achieve it at all (17). If the enforced pause between each coitus is longer than about 13 – 18 minutes the internal charge, probably endocrine in nature, which is provoked by the separate copulations, falls to almost zero between each coitus and the ejaculatory threshold (Rasmussen, 17) is never reached.

From our journal it is evident that 3 males displayed exclusively heterosexual behavior, 4 males exclusively homosexual behavior, 10 males both

homo- and heterosexual behavior and the remaining 3 males showed no sexual activity at all during any of the 2 minute observation periods.

As will be seen in the section: "Discussion of results and procedure" we do not recommend the procedure in this series of tests for future investigations.

## 5. A COMPARISON BETWEEN THE DIFFERENT TEST SERIES

We would expressly state that great caution must be exercised in comparing the four test series, as they had been conducted under experimental conditions which to some extent vary considerably on different points. Our object was to see if homosexual behavior could be provoked in animals of different ages, with varying heterosexual experience, under methodical changes in procedure etc. With these reservations we present in the left part of Table 9 the means for the number of shocks, the standard errors of the means, the standard deviations, coefficients of variation ( $V$ ) and the ranges for the four series of tests.

That, when all conditions are taken into consideration, the mean number of shocks for the first test series is not higher than 15, although the observation period for each test is 10 minutes, is due in the first place to the fact that the animals did not have any copulation with a female without a shock after each time. The fact that the mean is 23.8 shocks for the 2nd test series as against 14.4 for the fourth, in which the male also is allowed to accomplish the act of coition after each shock period, is at least in part due to the observation period for the former being 8 minutes, as against 5 minutes for the latter. The principal reasons for the number of shocks being so low for the 3rd test series, are the short observation period (5 minutes) and the fact that the animals had no copulations with the female without a shock after each time.

Table 9. A compiled table for the four series of tests. The means ( $M$ ), the standard errors of the means ( $\sigma_M$ ), the standard deviations ( $\sigma$ ), the coefficients of variation ( $V$ ) and the range are presented for the number of shocks and the number of attempts at coitus.

Series of test:	Shocks given				Attempts at coitus with male			
	1st	2nd	3rd	4th	1st	2nd	3rd	4th
$M$	15.0	23.8	12.6	14.4	3.3	16.8	18.1	10.9
$\sigma_M$	2.7	3.4	1.9	0.8	0.7	4.6	4.3	2.7
$\sigma$	10.5	11.8	7.4	3.6	2.7	16.0	16.7	12.0
$V$	70.3	49.4	59.2	25.0	80.9	95.4	92.0	109.6
Range	3-35	11-53	6-33	9-23	0-9	0-49	0-55	0-36



It will further be seen that the variability is lowest for the 4th series. On the basis of material at hand, it is impossible to conclude for certain that the variability is really lower with this method than with the other kinds of procedure, and there is therefore nothing to be gained by discussing such possibilities.

The majority of shocks are concentrated in the 2 first times the animals are tested. In the first and third test series, where the animals did not have any successful copulation which resisted the establishing of the inhibition, a relatively smaller number of shocks were given the last times the animals were tested than in the 2nd and 4th series.

As regards the temporal distribution the greatest number of shocks were given in the course of the first few minutes of the different observation periods in all series of tests. We thus see that it saves time to use short observation periods, and to put the animals into the apparatus several times (with suitable intervals) when the object is to establish an inhibition.

In the right part of Table 9 the means for the attempts at coitus with another male, the standard errors of the means, the standard deviations, coefficients of variation and the ranges for the four series of tests are set up side by side.

It will be remembered that the observation period was only 3 minutes for the first test series, while for the 2nd and 4th it was in all 12 minutes, and 10 minutes for the third test series.

Whether the difference between the 2nd, 3rd and 4th series is due to the different procedure used or the age of the animals, their previous sexual experience etc., is difficult to say. We see from the table that we here have phenomena of high variability. It would of course have been desirable to have had a larger number of animals in each of the test series.

The attempts at coitus with another male in all series of tests fall preponderantly in the first part of the observation period for each time the animals are tested.

## 6. DISCUSSION OF RESULTS AND PROCEDURES

In order to get a clearer estimate of the results obtained, we will place the test animals in 3 groups: one with no or a few attempts at coitus with the other male (group I), one in which the animals have some (group II) and finally one in which the animals have many (group III).

We must take the first series of tests separately as the period of observation was only 3 minutes in this test series. Here 6 animals having from 0 to 2 attempts fall in the first group (I), 2 animals each having 3

attempts belong to group II and 7 animals with from 4 to 9 attempts fall in group III (cf. Table 1).

Although the whole observation time was 10 minutes (5 separate periods of 2 minutes each) in the third test series and 12 minutes (6 separate observation periods of 2 minutes) in the second and fourth, we treat all these 3 series of tests together in this survey. 18 animals having from 0 to 4 attempts fall in group I. In group II there will be 9 animals, having 6 to 12 attempts, and finally 20 animals, having from 19 to 55 attempts at coitus, belong to group III (cf. Tables 4, 7 and 8).

In all we will consequently have 24 animals in group I, 11 animals in group II, and 27 animals in group III.

As only a few attempts have no significance, none of the animals belonging to group I can be said to have shown homosexual behavior. For some of the animals in group II there may also be extenuating circumstances: e. g. the animal is so eager that it has no time to explore whether the object mounted is a female or male.

But when a male tries vigorously from 19 to 55 times to accomplish the copulatory act with another male, such behavior can not be evaluated otherwise than as *definitely homosexual in character*.

Paying due regard to the short observation time in the first test series, we can state that we have *provoked such behavior in 27 of the 62 animals*.

With the above we by no means claim to have made these animals permanently "homosexual". It is only when in our apparatus in which they have received shocks that they will not mate with the female. Placed in the home cage with a receptive female after such an inhibition has been established, they will on the contrary copulate more often than usual. These investigations on experimental hypersexuality after "excitation et obstruction de la fonction sexuelle" will be found elsewhere (18).

Nor do we claim to have produced a desire or urge in the animals to mate particularly with another male rat: if we instead of a male rat had introduced e. g. a young guinea pig into the apparatus, some of the test animals would in all probability have mounted this object – just as Beach (2) was able to demonstrate in an ordinary cage after having given some male rats a few activation copulations with a receptive female.

We might also provoke the phenomenon – that a male would mount another male – if we like Beach (2, 5) gave a male a few activation copulations in an ordinary living cage, removed the female and then introduced a male. But in such instances the test male is not so eager, vigorous and persistent in his attempts at coitus with the other male as in the shock apparatus, and we have never, as was the case in the shock apparatus,

observed that a male has attempted the act of coition so eagerly and vigorously when a male was introduced into his living cage or when isolated from females, that he has achieved ejaculation on the back of other males.

This phenomenon, however, is a question of degree of sexual excitement: if we give a male in an ordinary living cage such a number of copulations with a receptive female that it is immediately *before* he may be supposed to reach the ejaculatory threshold (17), remove the female and introduce a male, it would probably also be possible to demonstrate the phenomenon that a male achieved ejaculation on the back of another male.

So far it cannot be said that we in this paper have demonstrated phenomena with our shock method which could not be demonstrated by other more simple procedures, except for one important aspect: we have shown that it is possible to establish inhibition in the male rat and that this inhibition towards females will lead the male to attempt to accomplish the copulatory act with other males *in the presence of receptive females*.

The mere presence of a receptive female and the two activation copulations before the test provoke a charge of the sex drive in our test males. The rat tries to relax this charge on available objects e. g. other males as in the present series of tests. The observable manifestations of the sex drive must supposedly depend on such a charge-discharge mechanism. A two component charge-discharge hypothesis of the copulatory act and ejaculation is expounded on the basis of previous experimental investigations (17).

It is evident from the treatment of the different series of tests that the average lapse of time before the first successful copulation (with intromission of penis) ranges from 28 sec. (1st series) to 81 sec. (2nd series).

The duration of this lapse of time – the activation time as it is called by Beach (2) or the provocative time by Carpenter (6) in his investigations on doves – depends on many factors. Perhaps the most important of these factors is the period of separation of the male from receptive females. The activation time, a previous copulation having taken place on the same evening, is usually only a few seconds (14). A male with no previous sexual experience at all is likely to have a long activation time. Experimental conditions and environmental influences being equal, hereditary factors also influence the lapse of time. The present author has previously demonstrated experimentally (15) that males descending from parents with high sex drive have a shorter activation time than those descending from parents with a low drive. The difference between the mean activation times of the two groups (164 animals) was 19 sec. with a standard error of 7.81 sec.

It is evident from previous data in this article, that the percentage of non-copulators varied largely from one series of tests to another. Other conditions being equal, this variability is also genetically determined. 5.6 per cent of the males descending from parents with high sexual drive did not copulate in the course of a 5 min. observation period compared with 14 per cent of the males from parents with a low sexual drive.

Only those instances where the riding movements were executed have been recorded as "attempts at coitus" with another male. After a successful copulation with a female, the male has a backward lunge (2, 8, 17, 19). In many cases the test males also show this characteristic reaction after an attempt at coitus with another male, in other cases the reaction resembled more closely the *attempts* at coitus – to be distinguished from successful copulation – often performed with receptive females. Beach (3) has given an excellent description of the different behavior patterns of the male when performing a successful copulation or an attempt at coitus.

With respect to not too old, sexually experienced males, the number of successful copulations, number of attempts at coitus and number of ejaculations are usually in the proportion 8:5:1 (14, 17). Perhaps the reactions where the test males had the backward lunge after an attempt at coitus with another male should have been recorded separately.

Not any male can be used as "the other male". Some defend themselves most aggressively and cannot be used, others that are not so difficult to find behave indifferently when mounted. To prevent "the other male" from copulating with the female, non-copulators or sluggish males must be used.

It was found to be "unfair" to attempt to "trick" the test males by using males displaying the female pattern of behavior when mounted (4, 5) as "the other male". Such males are in any case rarely met with. We would estimate the occurrence to be 1 or 2 in 1000 males.

Weaker shocks, using external resistances from 1,200,000 to 700,000 ohms, were also tried during the preliminary experiments. In these cases, however, some males accomplished the copulatory act despite the fact that they were given shocks.

It must be taken into account that the shocks were always administered after the male had placed his forefeet upon the female, the electric current thus being administered to the hind legs only. Muenzinger and Mize (10) have demonstrated that it takes considerably more current to produce a reaction when the animal rears on his hind legs than when he rests on all four. This phenomenon seems also to be evident from a previous experiment: rats on all four on the grill of the shock apparatus that had been deprived of food for several days, refrained from a food dish (12)

despite the fact that a much higher resistance, 1,666,667 ohms, was inserted, the voltage being the same (500 v).

The effective shock administered to the males in the present series of tests was approximately 0.75 to 0.80 milliampere (500 volts divided by the external resistances, 500,000 ohms + the resistance of the rat, about 150,000 ohms (16)). As this size of shock has been found suitable for the production of inhibition towards females, the use of a terminal pressure of e. g. 2000 volts and a resistance of about 2,500,000 ohms are suggested for further investigations. The large variability in the effective shock value (16), would in that case be avoided in future tests.

Experimental procedures other than those reported for the four test series were used in preliminary experiments. For instance, 1 female and 2 males ("the other male" with rubber socks) were present during the whole test and a shock was administered each time the test male mounted the female and no shock when he mounted the other male. Here the technical difficulty of the other male receiving a slight shock at the testicles, sometimes arose. Of course the testicles could be insulated in one way or another, but this procedure proved to be unsuitable in other respects: it seemed as if the negative condition toward the female was transferred to a variable extent to the other male when he was present in the apparatus when the test animal got a shock.

We have recently built a new model of the apparatus used in the present series of tests. Figure 6 shows a photo of this new model.

As observed on the photo the small compartments at the sides of the apparatus have been omitted. They have been of no use in the present series of tests or in the other investigations (11, 12, 14, 16, 18) in which the apparatus has been employed.

The measurements of the apparatus are the same, but as the front side a glass plate 5 mm thick is used. This plate is fastened by means of two hinges on its lower edge to a bar (3 cm wide and 2 cm high) which is screwed to the two wooden sides of the apparatus. Above, a similar bar is screwed to the wooden sides, and the glass plate is held in position by two snaps on the underside of the bar.

When pulled down, the plate rests on two brackets fastened to the lower bar. The plate is manipulated by a little knob (see photo).

The frame to which the wires are fastened is made of strips of bakelite, 4 cm wide and 2.5 cm high. This material insulates much better than wood applied with voltax (11). Between the two strips on which the wires are stretched there are two rigging screws. Their purpose is to permit tightening the wires when they have become slack after the apparatus has been used for some time.

The frame of bakelite rests on a wooden frame, 3.5 by 2.5 cm. In order to prevent this wooden frame from conducting the electric current, the bars of the frame are fastened to each other in such a way that the bars underneath the two common wires (11, 12) lie 1.5 cm lower than the two other lists on which the frame of bakelite rests.

In front and behind (not visible on the photo), 4 pieces of metal strips bent at right angles are screwed to the outer walls of the apparatus. By means of four clamps the apparatus, the frame of bakelite with the wires and the wooden frame are held tightly

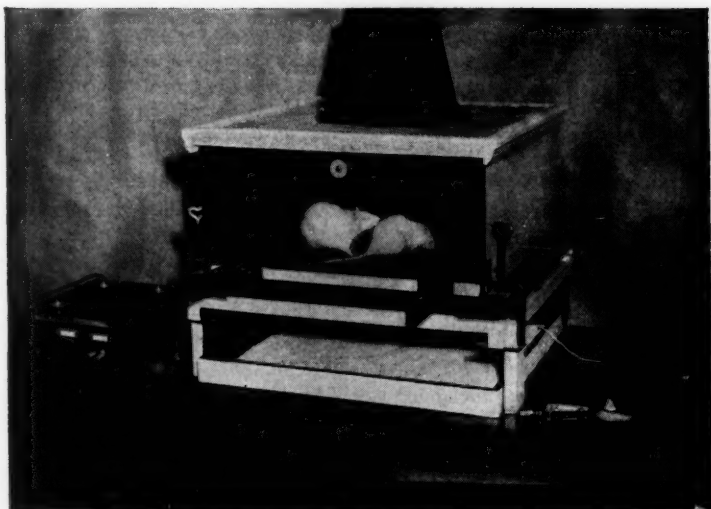


Fig. 6. The Shock Apparatus (new model).

together. The latter is supplied with 4 legs, 14 cm high. Underneath the apparatus is a tray with saw-dust.

The table on which the apparatus (and box with the electrical equipment) stands, can be folded up and used as a container for transporting the apparatus.

It will lead too far to evaluate and discuss the significance of the data on rats reported here for understanding homosexual behavior in man.<sup>1</sup> Only one thing should be pointed out: in man *positive* conditioning must be supposed to be of great importance. It is evident from the extensive investigations of Kinsey, Pomeroy and Martin (9) that homosexual contacts and relationships are very common during puberty. The sex drive fixes itself to members of the same sex, and this fixation may persist, consciously or unconsciously, for life.

1. In cooperation with a moving-picture company we have recently produced a film which shows the experiments reported in this paper. By writing to the author (address: Biological Institute, University of Oslo, Frederiks gate 3, Oslo, Norway) a 16 mm copy with text and speech in English can be procured. The price will be about 280 Norwegian crowns.

## SUMMARY

1. A method for giving rats electric shocks in an apparatus the entire floor of which is made of wires, is described.

2. In order to see if male rats given a shock each time they tried to mate with a female, would attempt to copulate with an introduced *male*, there has during 351 separate tests been administered a total number of 986 shocks.

3. A total number of 740 attempts at coitus with another male has been recorded. Among the 62 animals tested, 27 *showed behavior definitely homosexual in character* (from 19 to 55 attempts at coitus with another male in the 2nd, 3rd and 4th series of tests, from 4 to 9 in the first). 3 of these 27 animals *achieved ejaculation on the back of the other male*.

Eleven males displayed behavior which cannot be regarded as *unmistakably homosexual* - while 24 animals had below 4 attempts (in the first series below 2 attempts) at coitus with another male. As a male must be allowed to mount another animal a few times in order to ascertain whether the object is a receptive female, none of these 24 animals can be said to have displayed homosexual behavior.

4. Disregarding the 15 males in the 3rd series of tests, which were given no opportunity to demonstrate their heterosexuality after they had received shocks, 14 of the remaining animals showed *exclusively* homosexual behavior, 8 *exclusively* heterosexual behavior, 21 *both* homo- and heterosexual behavior and 4 no sexual activity.

5. There were 4 separate series of tests with different experimental procedures. The procedures used in the 2nd and 3rd test series demonstrate that homosexual behavior is less readily provoked in animals with a *strong* sex drive (receiving many shocks). They *strive for a heterosexual relaxation (17) of the drive as long as possible*. Successfully *defying the inhibition*, they finally copulate with the female and achieve ejaculation (2nd test series). The sex drive does therefore not need to seek relaxation through *homosexual* behavior.

But the procedure used in the first series of tests seems to demonstrate that when the inhibition has once been *firmly* established in animals with a strong drive, they will *engage more actively in homosexual behavior than animals with a low sex drive* (having received relatively few shocks).

Mention is made of experimental investigations to be found elsewhere (15) showing that the strength of the sex drive depends on genetic factors.

6. The great difficulties in drawing conclusions concerning man from experiments with animals are discussed. From a comparative point of view the homosexual behavior in the human being seems to be the result of an *interaction* of at least two of the following factors:

- a) strength of sexual drive, hereditarily determined.
- b) inhibitions (established by the environment) with regard to individuals of the opposite sex. The psychological factor.
- c) lack of sufficient opportunity to obtain a relaxation of the sexual need with individuals of the opposite sex, adequate to the individual hereditary demand for such relaxation. The sociological factor.
- d) hormonal imbalance, of importance first and foremost in those cases in which there is a *reversal* of the sexual pattern of behavior and whole personality. The imbalance may be hereditarily determined or the result of a disease e. g. tumors in any of the glands of internal secretion.



Until more experiments with animals have been performed and more data for humans secured by the taxonomic method (Kinsey), it is difficult to have a justified opinion as to the relative importance of the above factors.

7. We have in this paper shown experimentally that it is possible to provoke homosexual behavior in the male albino rat by punishing the animal when it tries to copulate with a female. In spite of the fact that receptive females are later present, many males will repeatedly and vigorously try to execute the act of copulation with other males after such a procedure.

The biological foundation of the above psychological factor in humans, mentioned under 6 b), has thus been demonstrated.

## ZUSAMMENFASSUNG

### *Experimentell hervorgerufenen homosexuelles Benehmen in männlichen Ratten*

1. Es wird eine Methode beschrieben, die es gestattet, männlichen Ratten elektrische Stösse in einem Apparat zu erteilen, dessen gesamter Boden aus Drahtgeflecht besteht.

2. Um festzustellen, inwieweit männliche Ratten, welche bei jedem Bedeckungsversuch an einem Weibchen einen Stoss erhielten, versuchen würden, ein in den Käfig zugebrachtes männliches Tier zu bedecken, wurden während 351 Einzelversuche insgesamt 986 Stösse erteilt.

3. Es wurden insgesamt 740 Versuche Coitus mit einem anderen männlichen Tier festgestellt. Von den 62 Versuchstieren zeigten 27 ein *seinem Charakter nach eindeutig homosexuelles Benehmen* (19 bis 55 Coitus-Versuche an einem anderen männlichen Tier in der 2., 3. und 4. Serie der Tests, von 4 bis 9 Versuche in der 1. Serie). Bei 3 von diesen 27 Tieren kam es *auf der Hinterseite des anderen Männchens zur Ejaculation*.

11 Männchen zeigten ein Benehmen, das nicht als unzweifelhaft homosexuell angesehen werden kann; 24 Tiere hatten weniger als 4 Coitus-Versuche (in der 1. Serie weniger als 2 Versuche) an anderen Männchen. Da man jedoch damit rechnen muss, dass ein Männchen einmal ein anderes Tier bespringen muss, um sicher zu sein, es mit einem empfängnisbereiten Weibchen zu tun zu haben, kann man von keinem dieser 24 Tiere behaupten, sie hätten homosexuelles Benehmen gezeigt.

4. Sieht man von den 15 Männchen in der 3. Serie der Tests ab, die keine Gelegenheit bekamen, ihre Heterosexualität nach dem Erhalten der Stösse zu beweisen, dann ergibt sich folgendes: 14 der übrigen Tiere haben ein *ausschliesslich homosexuelles Benehmen* gezeigt, 8 Tiere ein *ausschliesslich heterosexuelles Benehmen*, 21 ein *sowohl homo- wie heterosexuelles Benehmen*, und 4 zeigten überhaupt keine geschlechtliche Aktivität.

5. Es werden 4 von einander unabhängige Testserien mit verschiedener experimenteller Arbeitsweise durchgeführt. Das in den 2. und 3. Testserien angewandte Verfahren zeigt, dass sich homosexuelles Benehmen weniger leicht bei Tieren mit *starkem Geschlechtstrieb* (sie erhielten viele Stösse) hervorrufen lässt. *Diese Tiere erstrebten so lange wie möglich eine heterosexuelle Entspannung ihres Triebes* (17). Nach *geglückter Überwindung der Hemmung* bedeckten sie schliesslich das Weibchen, wobei es zur Ejaculation kam (2. Testserie). Der Geschlechtstrieb bedarf daher nicht der Entspannung durch homosexuelles Benehmen.



Das in der ersten Serie der Tests angewendete Verfahren scheint aber zu zeigen, dass, sofern eine Hemmung erst einmal *fest* eingearbeitet ist, es bei Tieren mit einem starken Geschlechtstrieb zu einem *mehr aktiven homosexuellen Betragen kommt, als bei Tieren mit schwächerem Geschlechtstrieb* (sie erhielten verhältnismässig wenig Stösse).

Es wird auf die an anderer Stelle veröffentlichten experimentellen Untersuchungen hingewiesen (15), aus denen hervorgeht, dass die Stärke des Geschlechtstriebes von genetischen Faktoren abhängt.

6. Die grosse Schwierigkeit, aus Tierexperimenten Schlüsse auf Menschen zu ziehen, wird besprochen. Vergleichsweise scheint das homosexuelle Benehmen beim Menschen das Ergebnis einer *Wechselwirkung* von mindestens 2 der folgenden Faktoren zu sein:

- a) Stärke des Geschlechtstriebes, erblich bestimmt.
- b) Hemmungen (durch die Umwelt entstanden) hinsichtlich der Individuen des anderen Geschlechtes. Der psychologische Faktor.
- c) Mangel an ausreichender Gelegenheit einer Entspannung des Geschlechtstriebes an Individuen des anderen Geschlechtes, entsprechend dem individuellen Bedarf nach Entspannung. Der soziologische Faktor.
- d) Fehlen hormonalen Gleichgewichtes, in erster Linie in jenen Fällen von Bedeutung, wo es sich um eine *Umkehrung* der geschlechtlichen Norm und der Gesamtpersönlichkeit handelt. Das Fehlen des Gleichgewichtes kann hereditär bestimmt sein oder das Ergebnis einer Krankheit z. B. Tumoren in einer der innensekretorischen Drüsen.

Solange nicht weitere Tierversuche durchgeführt sind und solange nicht mehr Material beim Menschen mit der taxometrischen Methode (Kinsey) gesichert ist, wird es schwierig sein, bezüglich der jeweiligen Bedeutung vorstehend genannter Faktoren eine wohlbegründete Auffassung zu haben.

7. Wir haben in dieser Veröffentlichung experimentell gezeigt, dass es möglich ist, homosexuelles Betragen bei der männlichen Albinoratte dadurch hervorzurufen, dass das Männchen gestraft wird, wenn es versucht, das Weibchen zu bedecken. Trotz der späteren Gegenwart empfängnisbereiter Weibchen werden viele Männchen nach solch einer Behandlung wiederholt und heftig versuchen, die Bedeckung an anderen Männchen durchzuführen.

Die biologische Grundlage des vorstehend genannten psychologischen Faktors beim Menschen, erwähnt unter 6 b), ist derart gezeigt worden.

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# MODEL CONSTRUCTION IN PSYCHOLOGY: A DEFENSE OF "SURPLUS MEANINGS" OF PSYCHOLOGICAL CONCEPTS

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## *1. The geographical map and the psychologist's model.*

The term "theoretical psychology" has today a certain prestige as a label on literature *about* psychological theories, characterized by attempts at launching and justifying norms for psychological research. In the present paper we shall focus on a few such specific norms, i. e., rules for theory construction and demands pertaining to research methods, especially prominent in current American literature. Our attention is then naturally directed toward what we shall call "theoretical models".

What is meant, then, by a "theoretical model"? What function have such models in psychological research?

Let us first turn to another science, geography, and to the trivial and well-known geographical map. The latter is a system of signs pretending to reflect the structure of a landscape. From one point of view it is a formal system, a delimited number of signs (lines, dots, colours, etc.) combined according to strict rules for their usage. But those signs also "correspond to" certain phenomena out in the landscape. The colour *blue* "stands for" *sea*, the contour lines "stand for" altitudes, etc. To the extent such an unequivocal coordination of map signs to observations in the landscape exists, there is an isomorphy or structural similarity between map and landscape. The rules for interpreting the signs are then *coordinating* or *semantic* rules. One simple rule is provided by the scale of the map. Others are more complicated: thus coordination of contour lines to observations of altitude must include auxiliary rules taking the tide into consideration.

The essential features of a research model can thus be diagrammatically represented as shown in Figure 1.

From one point of view it is a product of a cognitive process within the researcher, a structure of expectancies. From another point of view it is a formal system constituted by signs and *syntactic rules*, i. e., rules telling which signs belong to the system and rules for properly combining signs into complex constellations. From a third viewpoint the model is a "schematization of reality", with rules coordinating signs to observations.

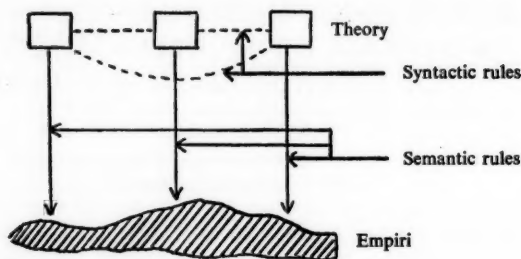


Figure 1. Essential features of a research model.

From this over-simple paradigm we can now try to approach theoretical models in psychological research. Some of the most well-known of them, like Hull's and Tolman's models of learning processes and Lewin's field-theoretical model, show a certain similarity to the map. Some of the signs included in their models (like habit strength, sign-Gestalt-expectancy and valence) refer to psychological concepts. The *syntactic rules* are then stated or implicit logical relations existing between concepts within the model and the *semantic rules* are the rules set up for anchoring concepts empirically. The latter branch off into research operations and ultimately into simple observations.

The purpose of model construction can thus be said to be a *true integration of theory (a structure of expectancies) and empiri (an aggregate of observations)*. But unlike most cartographers the research psychologist frequently finds himself standing on a most meagre and insufficient observational basis. His model is therefore to a much larger extent a structure of guesses, and his approach must be hypothetical-deductive. His task is fascinating because of its appeal to his phantasy. The danger, however, is that his phantasy may lead him astray so that his model remains a private intuition instead of a fruitful tool of research.

## 2. The intervening variable: A general and widely accepted semantic rule.

A considerable part of current literature on psychological theory construction is devoted to discussion of the danger mentioned above, with proposed rules to provide for an adequate coordination of models to facts. Representative are 5 articles in *Psychological Review* (1, 3, 4, 5, 6), and a key word in the discussion is the term "intervening variable."

The latter term was launched in 1936 by E. C. Tolman (7) as an essential item in his "operational behaviorism." As used by Tolman (7), it seems to refer to a *psychological concept unequivocally empirically anchored*

though not denoting a simple phenomenon (a stimulus or a response) accessible by direct observation. However, another great psychologist, C. L. Hull, was the one who perhaps contributed most to the further elaboration of this idea into an explicit general semantic rule. In his work *Principles of Behavior* (2) he devotes considerable space to this task, presenting the following simple scheme for the empirical anchoring of psychological concepts:

$$A - f \Rightarrow (x) - f \Rightarrow B.$$

The intervening variable ( $x$ ) cannot be observed, but is functionally related to an antecedent event ( $A$ ) and to a consequent event ( $B$ ), both  $A$  and  $B$  being directly observable. The term "intervening" thus refers to the relation between the model symbol ( $x$ ) and the observations by which it is empirically anchored. As a general semantic rule the simple intervening variable thus can be diagrammatically presented as in Figure 2.

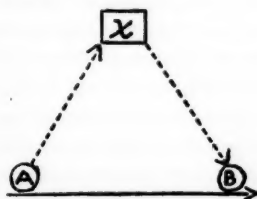


Figure 2. The simple intervening variable.

In his earlier and less explicit introduction of this rule Tolman (7) has given the following example of its application:

He is going to study hunger in rats. But hunger is itself a non-observable state within the organism. In order to take care of it in his research model he then has to introduce it as an intervening variable to be anchored in two sets of observations, (A) time since last feeding the rat and (B) motor activity shown by the rat. I. e., the state of hunger is itself a function of the duration of food deprivation and in turn has the consequence that the rat becomes restless, showing increasing motor activity. On the basis of a series of experiments the researcher is then able to draw a "hunger curve" (Figure 3), representing the functional relationship between  $A$  and  $B$ .

The concept 'hunger' has now been defined: When used later on in the more inclusive theoretical model it refers to the relationship between  $A$  and  $B$ , the intensity of hunger being represented by vertical location in the diagram.

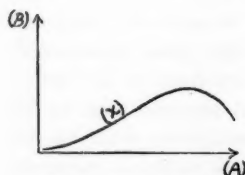


Figure 3. Hunger as an intervening variable.

Once a concept has been anchored in this way, it can itself be used (instead of a directly observable event) as one of the two anchors for another intervening variable. The latter is then called an intervening variable of the second order. By such a procedure one can proceed to build up complex theories like that of Hull, the model signs of which are (1) stimulus variables, (2) response variables, (3) simple intervening variables, and (4) intervening variables of higher orders.

### 3. *Emergence of surplus meanings.*

Seen in an historical perspective the great impact of Tolman's and Hull's ideas upon recent psychological research methods can perhaps most adequately be described as a forceful campaign for vaccination against introspectionism and anthropomorphism in psychology: When forced to translate every mental state into an intervening variable in a stimulus-response scheme, the research psychologist should have no chance of including vague and anthropomorphic conceptions in his theory. The latter is then supposed to be purely operational and behaviouristic, its intervening variables representing research tools without any subjective, mentalistic flavour at all.

Before long, however, some of the "vaccinated" research psychologists happened to be accused of cheating. Even though *introducing* their concepts according to Hull's strict rules, they seemed to forget about those rules when using the concept afterwards. Sometimes they tended to attach a *surplus meaning* to the model term, a meaning additional to that provided by the empirical anchoring of the term. If so, the intervening variable refers to the relationship between A and B *plus something more*. And in some cases this "something more" happened to be one or more of those dreadful anthropomorphic conceptions lurking in the psychologist's mind prior to the vaccination.

This sad fate reminds us of the old tale about the man and the annoying Puck: In order to get rid of his tormentor, the poor man had to move to another place. Great was his despair, however, when he suddenly heard Puck laugh at him from the top of the vanful of furniture.

Are research psychologists in general so heavily predestined to anthro-

pomorphistic thinking, that no vaccination seems to cure them? An inspection of current theoretical literature obviously suggests an affirmative answer to the above question. A distinctive stamp (cf. 1, 3, 4, 5, 6) is the still ardent struggle for vaccination, with systematic attempts to get rid of surplus meanings and thereby reduce concepts having the epistemological status of *hypothetical constructs* to "true" *intervening variables*. These attempts seem in part to be based upon the conviction that *any such surplus meaning is a foreign body in a scientific theory, either an "anthropomorphistic Puck" or an epistemological illusion (i. e., a reference to an assumed non-observable "reality")*. The demand for unequivocal empirical anchoring is then set up as a superordinate norm for psychological research: A concept, it is maintained, acquires its entire meaning from the rules coordinating it to observations.

#### 4. *Are surplus meanings necessarily "anthropomorphistic Pucks"?*

The hostile approach to surplus meanings sketched above is a highly biased approach, focussing mainly on the semantic rules attached to a concept without paying due attention to the model as a whole. The fact is that once we begin analyzing the meaning of a given concept, we are forced to recognize that it does not occur in a theoretical vacuum, but forms part of a more inclusive model. It therefore acquires its meaning from at least two sources, from (1) *the semantic rules* and (2) *the syntactic rules coordinating the concept to other concepts within the more inclusive research model*.

Let us try to illustrate this point by considering a psychological concept of motive. Obviously concepts of this kind acquire a major part of their meanings from syntactic rules, i. e. certain logical relations to other concepts are included as parts of their definitions, for instance in the following disguise:

"A motive is ----- *directing* behaviour toward a goal."

The above statement is suggestive of the dependence of "motive" upon other model terms. For one thing: By using such a concept of 'motive' we accept for inclusion in our model a characteristic of behaviour labelled "direction". *The concept itself then implies mapping human behaviour in terms of certain abstract characteristics, like direction and goal.*

At the same time the concept has certain semantic rules attached to it - at least in the sense that we are able to distinguish between a *class of phenomena that cannot be motives* and another *class of phenomena that can be motives*. In the latter is obviously included a state called "hunger". When we shall decide whether a given state of hunger is a motive



or not, however, we have always to resort to our syntactic rules and ask whether the hunger in this case can be adequately fitted into a more inclusive scheme as something directing behaviour toward a goal.

The surplus meaning of some psychological concepts may thus be broken down into two intimately related components, into a component of *explicit syntactic rules* and a somewhat more hazy component of *expressive meaning*, i. e., an implicit preference for a certain system of classification reflecting the researcher's general way of looking at psychological phenomena.

### 5. A plea for more flexible models.

The above vague and general considerations lead us to a series of problems systematically avoided by the more ardent preachers of operationalism in psychology. We are induced to look at the research process from another angle, taking into consideration the expressive and exploratory aspects of the concepts as products of a cognitive process in the mind of the research psychologist. Let us therefore try to approach his theoretical model as an integrative part of a learning process.

The initial phase can then be described as follows: The research psychologist is confronted with an aggregate of psychological phenomena appearing rather chaotic at first sight. But gradually – frequently during a series of arm-chair explorations and of more or less casual observations in the field – he begins to find some sort of structure in what appeared to be chaos. He has the experience of getting a general view of the field, mapping outlines of distinct woods where he previously saw nothing but single trees. This experience of structuring may be described in part as an intuition, in part as a deliberate simplification and – maybe – a utilization of knowledge from other fields. The latter aspect – if existing at all – is revealed when the subjective experience of having structured the field is reflected on a bit of paper, for instance in a diagram as shown in Figure 4.

The research psychologist tells us that this diagram is a provisional and primitive, though general theoretical model for inquiries into a complex of psychological phenomena labelled “ambivalence”. The circle represents a

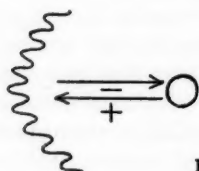


Figure 4. Model for studies of ambivalence.



(social) object, the arrow marked - stands for a psychological force pulling the organism away from the object, and the arrow marked + represents a force working in the opposite direction. The spiral, finally, stands for a characteristic of the behaviour of the organism, viz. that alternation between approach and avoidance that in the psychologist's opinion will occur when the two forces are of approximately equal strength.

To what kind of observations in the jungle of psychological processes do these model signs refer, then? - Obviously the theorist cannot present rules for empirical anchorage of the intervening variable type. Because his model pretends to present a very general structure and cover a broad field of research, it can be coordinated to a whole series of observations, apparently representing quite different psychological phenomena.

Concretely: The model may be used as the theoretical basis for experiments on rats. The model sign plus-force can then be coordinated to relatively simple observations, viz. observations of the behaviour of a calibrated spring withholding a hungry rat in her locomotion toward food. The minus-force, furthermore, is assessed by the same kind of simple observations when the rat is withheld from running away from a painful electrical grill. On the basis of such measurements the psychologist may now be able to make a prediction of the hungry rat's behaviour when placed in a box where food is located on the electric grill: If the observed forces have been found to be approximately equal, his prediction will be that the rat will tend to oscillate between approach and avoidance at a certain distance from the grill.

At the same time the model can be applied to an apparently quite different aggregate of psychological phenomena like a *complex interpersonal relationship*. The organism is in this case a person P, the object another person O, and the arrows refer to positive and negative emotional ties of P to O to be assessed by various measurements of emotional attachment. If observations demonstrate a balance of the two forces, the prediction must be ambivalent behaviour. The spiral does not in this case refer to alternating physical locomotion of P towards and away from O, however, but maybe in part to what might be called "oscillating verbal behaviour", i. e., a persistent oscillation between praise and reproach in P's mention of O.

In model construction of this type, a given model symbol (like the plus-arrow) thus branches off into a hierarchy of semantic rules as shown in Figure 5.  $R_1$  is then a general rule, stated in terms of abstract characteristics of behaviour,  $R_{11}$  may be a rule pertaining to all possible coordinations of the model sign to rat behaviour, and  $R_{111}$  to  $R_{11n}$  may be speci-

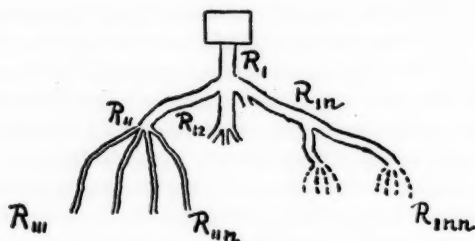


Figure 5. Flexible empirical anchoring.

fic operationally phrased rules for details of observations. Another main branch ( $R_{12}$ ) may pertain to a certain sector of social behaviour, ultimately branching off into detailed observations of simple emotional expressions.

What is obtained, then, by such a hierarchical anchoring of model signs? One main credit is the inherent flexibility of a hypothetical-deductive system, providing for a more varied interaction between theory construction and empirical research than any strict and narrowly operational methodology allows. Thus discrepancies between predictions and facts do not simply "disprove" hypotheses, but can lead to any one of the following three types of revisions:

(a) A revision of semantic rules. (I.e., the psychologist finds in one case that the coordination of model sign to observations was inadequate. Consequently he sits down to construct new research instruments to assess the phenomenon aimed at by the more general semantic rule.)

(b) A delimitation of field of prediction. (I.e., a revision of type (a) is out of question and the psychologist concludes that his theory does not hold for the entire domain of observations that his model originally intended to cover. The "law" may hold for rats but not for man or vice versa.)

(c) A revision of the model syntax. (I.e., he has reason to believe that the stated logical relationships between model signs are not sufficiently isomorphic with the observed relationships between the psychological phenomena in question. Consequently he sits down to examine his entire model in the light of the new observations in order to bring about isomorphy.)

6. *Concluding remarks: The role of "prescientific knowledge" in psychological research.*

Apart from its possible spontaneous emotional appeal to research psychologists who worship some freedom of imagination, the flexible type of models sketched above has the essential advantage that *it takes care of and in a way legalizes what may be called the research psychologist's "prescientific knowledge"*.

We do not assume that our account of the initial stage of the research process is totally unbiased: Other descriptions may justly emphasize strict induction to a much larger extent than we have done. The problems pertaining to the genesis of the researcher's initial pre-scientific knowledge of the phenomena he is going to investigate are in this case irrelevant, however. They are irrelevant to the extent that we agree that the researcher, before he enters the laboratory where the data for his experimental report will be established, is provided with *a certain knowledge that, though frequently more diffuse and primitive than scientific knowledge, does not differ radically from the latter in any way.*

Prescientific knowledge is thus not necessarily a product of revelation but may at times be stated as assertions about structural similarity between apparently different aggregates of phenomena, based upon observations. A systematic elaboration of semantic rules then frequently reflects the process of induction having led up to it. Because the psychologist's experience outside the laboratory has been so rich and varied, however, the hunched and abstractions based upon it cannot be pinned down into operational terms to be tested by a single crucial experiment without major residuals. Therefore the meaning of a given model sign frequently has to be only partially delimited, i. e., the stem of the tree of semantic rules ( $R_1$  in Figure 5) must include a residual of haziness, representing that part of the researcher's knowledge (or empirically based hunches) that cannot immediately be taken care of in terms of detailed rules for observations. If this residual of as yet inexplicitly stated but vaguely anticipated semantic rules is eliminated by a single operational definition (in part dictated by narrowly pragmatic considerations concerning available research instruments) the researcher will sever his contact with the universe of extra-laboratory observations in a most unfortunate way.

*The issue is not pro and contra anthropomorphism.* As every other branch of science, psychology must proceed as a continuous expansion and refinement of non-scientific observations and knowledge. The partial delimitation of the meaning of model signs is a device furthering such a

development, whereas strict operationalism is not. Indeed, by tacitly assuming a strange dichotomy between a region of pre-scientific revelations and a realm of "scientific" (i. e., operationally defined) terms, the preachers of operationalism ultimately stand out as the mystics of modern scientific psychology. If lived up to, their doctrine will gradually remove all more flexible models from psychological research and thereby severely hamper the struggle for theoretical integration.

Thus in spite of its areas of haziness and consequent difficulties pertaining to unequivocal testing of hypotheses, the flexible model is a device by which we can try to bridge crucial gaps between fields of observation (like a rat laboratory and the therapist's room). It is also a device by which significant extralaboratory insights can be transformed so as to initiate scientific research. Suppose we happen to find a psychologist who outside the laboratory has made observations that may in the future prove as crucial to the development of psychological knowledge as those made by Newton in the orchard turned out to be to natural science. This sounds very unlikely, indeed. But the fatal fact in this connection is that we shall probably never know whether he is a potential Newton or not *if we at this stage of development force his model into a strait-jacket of operational definitions.*

All the above considerations converge in a defense of surplus meanings of psychological concepts as parts of flexible research models. Psychology is a young science and extralaboratory observations still provide an enormously rich basis for induction when compared with laboratory research (parts of which may by our future judges even be considered psychological alchemy). Therefore we urgently need a legitimate device by which our prescientific knowledge can adequately be taken care of. By legalizing and controlling our imagination we are probably far better off than by denying its existence or by excluding it altogether from the "decent", scientific part of our work. As a general norm for research, then, juxtaposed to the demand for unequivocal empirical anchoring, we assert: We should construct our research models so as to provide for optimal use of prescientific knowledge.

Our ultimate aim is of course one unequivocally empirically anchored psychology. If we are going to prevent an irreversible splitting up of our science into a number of small and conceptually isolated fields we must, however, at the present stage in part also invest research efforts in attempts at building up systems of higher order concepts and be tolerant toward *potentially fruitful conceptual ambiguities.*

## SUMMARY

The present paper represents a critique of a current trend in theoretical psychology culminating in a demand for unequivocal empirical anchoring of concepts as a super-ordinate norm for psychological research. We have tried to point out certain serious consequences of such a programme. Firstly: By searching for the "meaning" of a given concept exclusively in terms of explicit rules coordinating concept to observations we fail to take into consideration that part of its "meaning" may be syntactic, i. e., pertaining to rules formally relating the concept to other concepts within the same model. Secondly: A residual area of ambiguity may constitute a means whereby highly useful pre-scientific psychological experiences can be transformed into fruitful scientific research.

## ZUSAMMENFASSUNG

Der vorliegende Artikel ist Ausdruck für eine Kritik von aktuellen Tendenzen in der theoretischen Psychologie, die in der Forderung kulminieren, dass die eindeutige empirische Verankerung von Begriffen die oberste Regel für psychologische Forschung sein soll. Wir haben versucht gewisse ernsthafte Konsequenzen eines solchen Programmes anzudeuten. Erstens: Dadurch dass man nach dem "Sinn" eines gegebenen Begriffes ausschliesslich mit Hilfe von solchen Regeln sucht, die Begriffe Observationen zuordnen, so unterlassen wir es zu beachten, dass ein Teil ihres "Sinnes" von syntaktischer Art sein mag, d. h. dass er unter solchen Regeln fällt, die den Begriff formell in eine Relation zu anderen Begriffen in dem selben Modell stellen. Zweitens: Ein restliches Gebiet von Unklarheit mag als Mittel dienen, durch welches vorwissenschaftliche psychologische Erfahrungen in fruchtbare wissenschaftliche Forschung umgeformt werden können.

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## THE EFFECT ON AN ESTABLISHED PERCEPT OF A PERCEPTUAL PROCESS BEYOND AWARENESS

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In an investigation at Harvard University the senior author of this paper and George S. Klein introduced a tachistoscopic experiment, the purpose of which was to explore the perception of two contradictory stimuli presented on the same area of a screen immediately after each other. Outlines of three faces were used as stimuli, all of them alike except for the mouth. In the face presented first the mouth might be a half-circle, the open ends pointing upward (happy face); in the face presented immediately after the first the mouth was a horizontal line (neutral face). If the first face was, for example, exposed for .01 seconds and the second face for .05 seconds, the first face was most often extinguished by the second, as could be predicted from experiments by Ehrenstein, Werner and Cheatam (2, 3, 18). Several subjects (Ss) did not see the first face until its exposure time was prolonged up to .15 or .20 seconds. But it also became evident that the first face, though not seen by S, (or, rather, a perceptual process representing a pre-stage of the conscious perception of that face), nevertheless affected the perception of the second face in many ways, as, e. g., when the neutral second face was reported as gradually more happy the more the exposure time of the first (happy) face was prolonged. This influence on a conscious percept by a percept not conceived by S proved statistically significant in many of the experimental series conducted at Harvard.

In other words, here was a method with immediate implications for the problem of the effects of subliminal stimulation and, more generally, of the development of a percept. It may be sufficient to point out here that there are few if any studies dealing directly with the significance of perception beyond awareness by measuring its effects on a succeeding, consciously established percept. The problem of perception and awareness has, on the whole, attracted the attention of rather few psychologists (1, 8, 9, 10, 11, 12, 13); and in psychoanalysis as well as in gestalt psychology a percept is supposed always to be conscious. The wider problem of the development of a percept has been studied of course: by the method of *Aktual-genese*, by change in contour and illumination, etc.. In many studies of this kind, This study was supported by a grant from Magnus Bergvalls Stiftelse, Stockholm, Sweden.

response at various levels of conscious organization is summarized to represent the perceptual development. The method employed here, however, seems to be a still more valid attack upon the problem of development, since it makes possible objective observation of a perceptual process even below the threshold of awareness.

These important implications of the study at Harvard for a theory of perception have led us to arrange similar experiments in Lund. The experiment reported below constitutes part of a larger program drawn up to study different aspects of the genetic problems in perception, the theoretical prerequisite being that every percept (stimulus) should be considered the outcome of a more or less extended micro-genetic process. We will deal here with the more specific problem of the effects of a perceptual process not represented in consciousness (i. e., not as outside reality) as measured by the change observed in a conscious percept appearing immediately afterwards. Other experiments within this general area are planned by George S. Klein at New York University (6).

#### PROCEDURE

Stimuli were presented in the electronic tachistoscope of the University Psychiatric Clinic. This tachistoscope has three projectors allowing three pictures to be exposed immediately after each other or with intervening pauses. Exposure times may be varied from 3 milliseconds upwards. The pictures appear on a milk-glass screen, their size being about 40 by 40 cm. Ss are placed in an armchair at about  $1\frac{1}{2}$  m. from the screen. A diffuse circle of light on the screen orients them as to the middle point of the picture that is going to appear next; since this illumination is very weak it will vanish once the picture is flashed on the screen.

Only two stimuli were used in this experiment, the line design and the square constituting the well-known illusion shown in Fig. 1. In the first part of the experiment the square was presented alone; in the second part the line design was immediately preceding. Since, from the stimulus point of view, the purpose was to study the impression produced on the shape of the

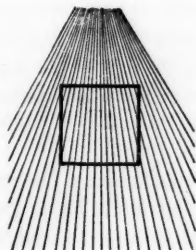


Fig. 1. Stimulus 1 (the line design) and stimulus 2 (the square) presented together.



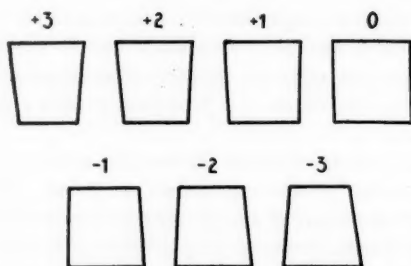


Fig. 2. The scale used for judging the second stimulus.

square of a line pattern not consciously perceived by S it was necessary to arrange the experiment so as to prevent S from discovering the first picture already in its early presentations. When about one third of the experimental program was finished, it became evident that nearly all Ss had become aware of the line pattern very early. Therefore the line stimulus was covered with a grey, transparent film and thus prevented from easy discovery with short exposure times.

The tachistoscope broke twice during the experiments as a result of short-circuiting. It proved impossible to match precisely the exposure times of the first picture in series conducted before, between, and after repairs. Therefore we are going to use scale values instead of time values. One scale point corresponds to about 1 millisecond for the first picture and to about 2 milliseconds for the second. Since the two quotients between scale values and time values are constant throughout the experiment for any single S, we are entitled to base calculations on figures representing intra-individual comparisons. But we refrain from direct inter-individual comparisons. It is self-evident that the relative increase in exposure times is the same for all Ss.

O introduced S to the task by presenting stimuli not belonging to the experimental series at short, medium, and long times, and told him to keep his eyes on the fixation circle and to report everything he saw flashed on the screen. Thereafter the square was exposed at a scale value of 3. If S did not report a square, or if he was not sure that he had seen a true square, the square was presented again at scale value  $3\frac{1}{2}$  etc., until it was correctly recognized. Most Ss reported the square at 3. Then O handed S the scale shown in Fig. 2 asking him henceforth to report all figures in terms of this scale, informing him that the square he had just seen represented zero on the scale. Furthermore, O underlined that S should report *everything* he could see on the screen.

In the following control series the square was presented alone a further five times at the exposure accepted by S for full recognition. Then, the first picture (the line pattern) was exposed immediately before the square



at gradually prolonged times, first at scale values 3,  $3\frac{1}{3}$ , 4,  $4\frac{1}{2}$ , etc., up to 20, then, because the graduation became less differentiated, at 22, 24, 26, etc.,. As soon as S discovered the lines behind (or immediately before) the square he was thoroughly questioned about them so as to make sure whether he had seen a lined background earlier than reported. If there was any doubt concerning the moment when S had first observed the lines his results were excluded from further treatment. Ss who discovered the lines after only five or six exposures were also excluded, i. e., practically all Ss tested before the first picture was covered with a grey film.

24 Ss are left for statistical treatment below, all of them graduate students at the university. There are 16 women and 8 men among them. Their average age is 26.3 years.

### RESULTS

The number of exposures needed to discover the line pattern varies considerably. Most of our Ss, viz. fourteen, stop at less than 35, which corresponds to scale values below 20. Among the other Ss, eight required up to 48 exposures (46 on the scale), and two Ss another 7 or 8 exposures (60 and 62 on the scale). The fact that some few Ss needed more time than the rest of them for objective perception of the square cannot account for these large differences; neither can they be due to such slight inter-individual changes in basic exposure time as were possible because of intervening repairs of the apparatus. But similar variations in the Harvard experiments suggest substantial individual differences in recognition threshold, or in tolerance for the introduction of unexpected stimuli.

Introductory information on the experimental results is yielded by an inspection of the frequencies with which various judgments of the square

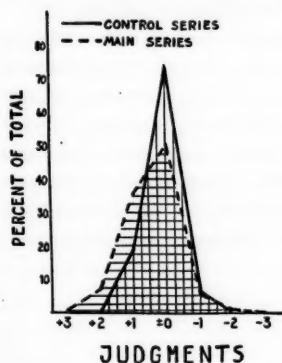


Fig. 3. Relative frequencies of scale values used in judging the square.

were used. Fig. 3 reveals that the square is judged as a square (0 value) in nearly 75% of the control (single) presentations. But in the main series, where the lined pattern was flashed immediately before the square, the distribution of judgments has changed. Only 50% of the presentations produce square responses. Instead, there is an increase in positive judgments, i. e., judgments expected if Ss had been aware of a lined background. But the relative frequency of negative responses has hardly decreased.

Calculating the arithmetic mean of the control series for each S (using the judgment figures) and thereafter the mean for the whole group based on these individual means, we obtain the value of  $+.14 \pm .056$ . There is thus a weak tendency toward positive responses. Since it will be important to know whether this tendency appears toward the end of the first series, i. e., whether positive responses are solely the result of repeated stimulation, we have estimated the regression in this series by means of the formula  $(2x_5 + x_4 - x_3 - 2x_1)$ , where  $x_1$  refers to the first presentation,  $x_2$  to the second, etc.. The mean of the whole group is  $-.17$ . If anything, the group tends to give less positive responses as stimulation is repeated.

One could object, however, that those of our Ss who perceive a change in the direction of more positive quadrangles also account for most of the positive responses in the main experiment, i. e., the shift toward positive judgments in this part of the test shown above should nevertheless be ascribable to the effect of repetition. Using for each S the mean of the main series (where the line design is present) subtracted by the mean of the control series we have calculated the arithmetic mean of these values for 6 Ss with a positive tendency in the control series (the mean is  $+.18$ ), for 7 Ss with a negative tendency ( $+.21$ ), and for the rest of the sample ( $+.25$ ). Evidently there is no reason to suspect that the increase in positive judgments in the main experiment is the result of the effect of repetition.

The main experiment will be handled most easily if we split the data into three parts for each S according to whether they emanate from the first, middle or last third of the experiment. The means of these parts are corrected for the mean of the control series, i. e., the control value is subtracted from them. Since there are never less than 12 presentations, and most often more than 20, it is of no importance that the number of presentations used by an S cannot always be evenly divided by three. The three corrected means thus obtained ( $S_1, S_2, S_3$ ) represent three consecutive phases of perceptual development and will allow comparison between Ss.

Results of these calculations are presented in Table I.

Table I: Corrected Means of Judgments of a Square in the First, Middle, and Last Parts of a Tachistoscopic Experiment, where Ss were not Aware of a Preceding Line Design Presented at Gradually Prolonged Exposures.

Means of		First third	Middle third	Last third
		$+ .145 \pm .1000$	$+ .379 \pm .1039$	$+ .329 \pm .1010$
Comparison with zero	t	1.45	3.65	3.23
	p*	<.10	<.001	<.005

Note: Plus-judgments go in the direction of the illusion created by the two stimuli perceived together as in Fig. 1.

\* One-sided test.

In the first part of the experiment the lined pattern evidently exerts little influence on the judgments. During the following presentations there is an increase in positive judgments, however. The means of  $S_2$  and  $S_3$ , as compared with 0 as a base line, are strongly significant. It is of special interest to notice that the influence of the first perceptual process on the second is even greater in the middle than in the last part. This trend will enable us to refute here the possible objection that, in one way or other, Ss have seen the lines behind the quadrangle and only therefore reported deformations of the square. The line pattern was, of course, most likely to be seen in the last third of the experiment, where exposure times were relatively longer. Nevertheless, the strongest influence was not registered in this portion. Moreover, because of the exclusion of all doubtful cases we may at least state that the square changed as a result of percepts not consciously registered by Ss.

The problem of individual differences can be only lightly touched upon in this paper. Though most of our Ss respond more positively toward the middle or end of their series, there are some few Ss in whom this inclination is reversed. Other Ss are hardly affected at all by the introduction of the lines; they react throughout the entire experiment in the same way as in the control series. Even negative reactions are to be found. However, the only deviating group of any number worth considering for calculations are those Ss who did not change their response when the first stimulus was introduced.

21 of 24 Ss were tested in other tachistoscopic situations after the square experiment had been finished. Stimuli were presented in the same way as in the experiment reported above. In one of these experiments a car was introduced as first stimulus before a street scene, in another experiment the interior of a room was substituted for the street scene; in a third experiment

the first stimulus was a baby and the second stimulus a family group around a blanket, and in the last experiment a group of three rats of human size was substituted for the family. While the length of the exposure of the second picture was kept constant at full recognition time, the exposure of the first picture was prolonged until S had reported it objectively.

In  $S_2$  and  $S_3$  seven of the Ss obtained means (after correction) of  $< +.15 > -.15$ . We consider their series as relatively unaffected by the line design. Hypothesizing their reactions as a sign of isolation of the two percepts, the percept beyond awareness not being allowed to influence the established, conscious percept, how would we predict their results in the following experiments? It seemed to us self-evident that, especially in series with incongruent stimuli, they would not accept the first stimulus until it had been isolated from the second, i. e., they would tend to recognize the double exposure before they accepted the meaning of the first stimulus, or, they would see the stimuli alternatively to begin with. Of the unaffected Ss 6 showed such reactions, mainly in the second and fourth experiments, of the remaining 14 Ss only 4. Comparing the groups, we get a  $\chi^2$  of 4.03, corrected for continuity ( $P < .05$ ).

#### DISCUSSION

Before the concept of unconscious mental processes was introduced by Freud, it would have been inconceivable to hypothesize a perception outside awareness. But even for Freud, the quality of consciousness seems to be indispensable for the ego-function of perception. In his summary of the psychoanalytic theory of perception (5), he concludes that as long as the system perception-consciousness is cathected from within it receives perceptions which are accompanied by consciousness and passes the excitations on to the unconscious mnemonic systems. But as soon as the cathexis is withdrawn, consciousness is extinguished and the function of the system comes to a standstill. Though the Freudian system of perception, contrary to contemporary models, is part of the self-regulating dynamic system of the whole organism, for Freud, as for the structural psychologists of his time, a percept is either instantaneously present in consciousness or does not exist at all.

During the last decades interest in more liminal forms of perception has increased. Present research in the reflection upon perception of central processes, for instance, is very often based on experiments where clear, objective percepts are made impossible by diminishing the time of presentation, the brightness of stimuli, etc.. However, in most perception studies, as in the theories of gestalt psychologists, percepts are accepted as

immediately present. The "dynamic" question about the origin of a percept is most often a question of factors converging into it at the moment of its appearance, i. e., of stimulus conditions, momentary set, retinal structures, etc., but not a question of how the percept evolved – of the genetic prerequisites of the perceived objects.

Felix Krueger (7), Sander (14,15), and many others studied the micro-genesis of percepts, i. e., the momentary development up to a final structuration. These studies, though long forgotten because of the verbose theory accompanying them, have been recently followed by similar experiments by Stein, Framo, Smith and Kragh, and many others (4, 16, 17). A renewed interest in subception, as, e. g., studies in automatic discrimination without awareness and other studies referred to in the introduction, seem to reveal a shift of focus of perception studies from the ready product of perception (stimulus) to its genesis the moment before its final structuration. If we assume that percepts arise as the result of a momentary development, an assumption upon which this paper is based, experiments on perception outside awareness should be the prerequisite for a complete theory of perception.

The results of this study force us to assume the existence of pre-stages of conscious percepts (of which Ss are not aware) revealing themselves only through their influence on proximate, conscious percepts. The effects of these perceptual processes beyond awareness are negligible to begin with, but increase as exposure times are prolonged, i. e., the quadrangle seems to change so as to become more and more prepared for the appearance of the line design as a conscious percept. However, after a maximum in the middle part of the experiment the illusion seems to diminish slightly before the first stimulus is recognized. We know that the illusion used here, if shown repeatedly to Ss, would tend to diminish gradually and finally to disappear. Our results seem to indicate that such a process of objectification may start before Ss are aware of the background. It might be relevant to this conclusion that when confronted with both the stimuli presented together after the experiment, 58% of our Ss reported the square as slightly deformed (+1) and 42% as a real square.

As always, individual differences are the basis for testing hypotheses about the impact on perception of central processes. We have only few results to offer here. Ss who deviate very little from square responses seemed to be more disposed than others to isolation mechanisms in perception, i. e., instead of accepting the first picture (in another set of experiments) as a new object in the second established percept, they tended to see the first stimulus as independent of the second. Even negative responses, contrary

to the illusion expected, were rather frequent, at least not less frequent in the main series than in the control. These negative responses might be a sign of overstressed resistance to the effect of the "line-process", a resistance very much resembling the defense mechanism of reaction-formation. There are also individual differences as to the position of the maximum effect of the perceptual process outside awareness: most often it appears in the middle part, but may also appear in the first or last parts. We hope to be able to study these and other developmental differences more closely in experiments planned to follow the one reported here.

### SUMMARY

In this tachistoscopic study a line design and a square are presented in rapid succession. Together they constitute a well-known optical illusion where the lower side of the square looks shorter than the upper side. The exposure time of the square is kept constant at full recognition time, the exposure time of the line design is gradually prolonged. The experiment is confined to that part of the exposure series where Ss are still unaware of the first picture.

After each exposure Ss judge the form of the quadrangle (square) according to a scale where minus-figures and plus-figures denote gradual decrements of the upper and lower sides respectively and zero represents a true square. Most judgments go in the positive direction (as compared with judgments in a previous control series where the square is presented alone) i. e., Ss judge the quadrangle as it appears in the illusion. The deformation seems to be strongest in the middle of the exposure series. This change in the perception of the square is not the result of mere repetition of presentations but of a line design of which Ss are not aware, or rather of pre-stages of the perceptual process eventually leading up to conscious perception of the lines. Individual differences, especially with respect to isolation mechanisms, are also studied, and the implications of the results for a genetic theory of perception are discussed.

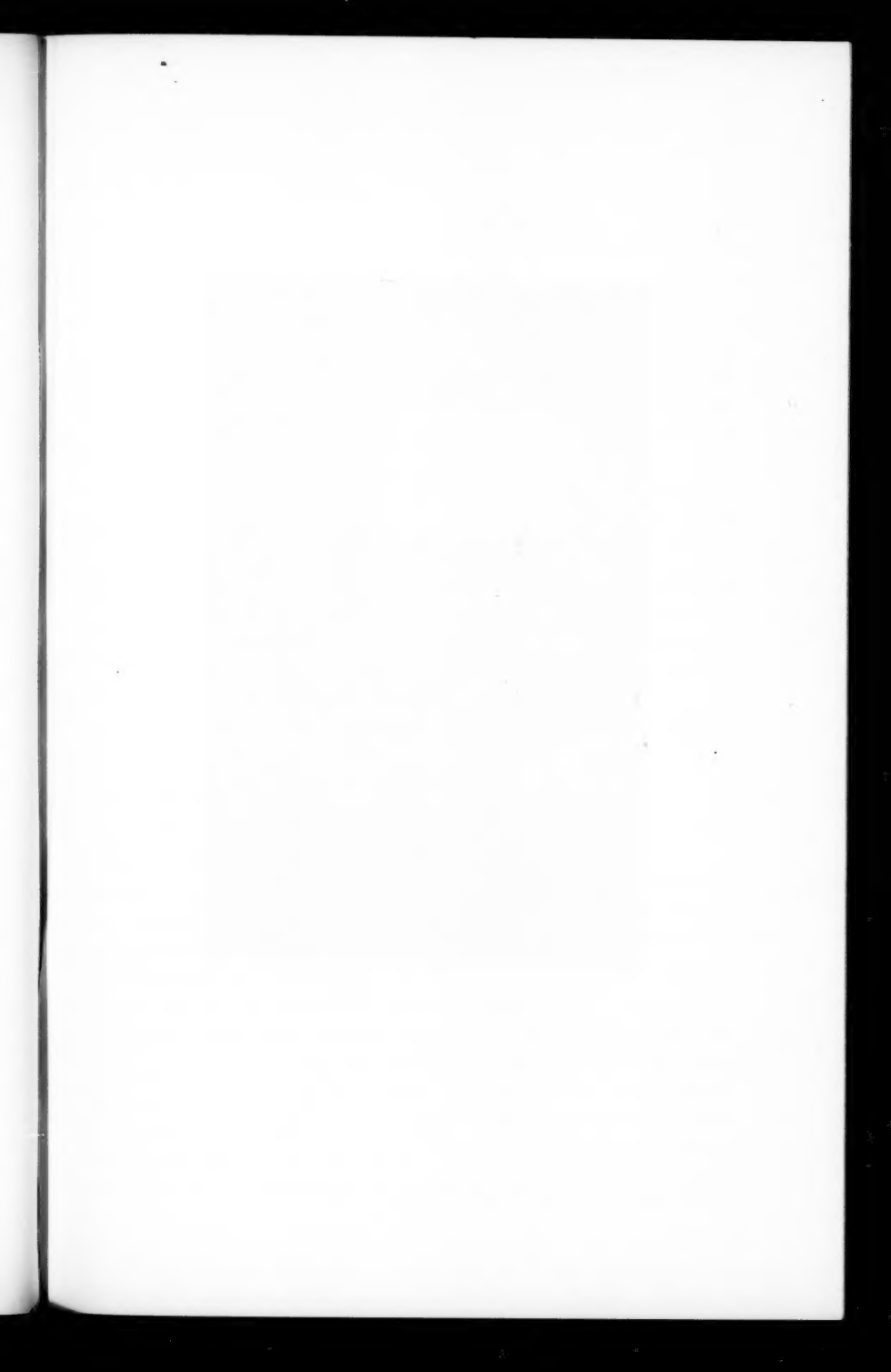
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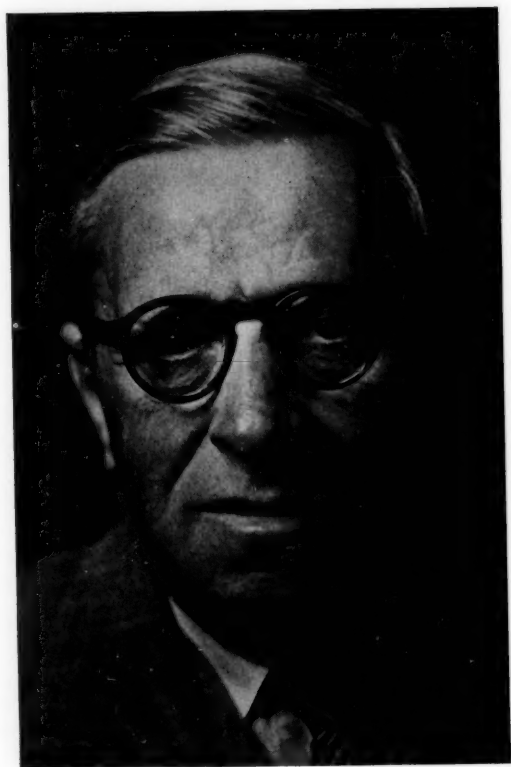
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PROF. DR. G. RÉVÉSZ

## IN MEMORIAM GÉZA RÉVÉSZ

Dr. Géza Révész, emeritus professor of Psychology at the University of Amsterdam, founder and editor of *Acta Psychologica*, died from a heart attack on August 19, 1955. Thus came the end of a distinguished scholar and a remarkable man, whose many contributions attest an unusually wide scope of interests, a rare combination of originality and thoroughness, an unwavering dedication to the ideals of science.

Révész was born in Siofok, Hungary, on December 9, 1878. He studied law at the University of Budapest. Immediately after having obtained his degree he went to Germany, in order to obtain the best training in psychology then available. He attended courses in several Universities, but his main teacher was Georg Elias Müller, from whose laboratory in Göttingen so many eminent psychologists have come forth. After the completion of his studies he returned to Hungary, where in 1908 he was appointed a "Privat-Dozent" at the University of Budapest; in 1918 he became a professor at the same University.

In 1920 he left Hungary and came to Holland, where he devoted himself to scientific research, and set up a consulting practice. Since 1923 he gave lectures in industrial psychology at the University of Amsterdam. Here he was in 1932 appointed as professor of psychology and director of the Psychological Laboratory. In 1950 he retired, having reached the statutory age.

During the first stages of his scientific career Révész' main publications were in the field of visual perception. But soon his interest was attracted by a problem area in which, by virtue of an exceptional combination of gifts, he seemed predestined to play a leading role: the psychology of music. In 1913 already his "Grundlegung der Tonpsychologie" appeared, in which for the first time he exposed his theory of tone perception, based on the distinction between pitch and tonal quality, and therefore called the "two component" theory. In this first major publication we find already those qualities, which would remain distinctive for all his later work: refined phenomenological analysis, careful and ingenious experimentation, clear conceptualization, judicious argumentation. His enduring interest in music manifested itself in a long series of other studies, many of which were summarized in his "Introduction to the psychology of music" (Dutch; 1944),

also published in German under the title "Einführung in die Musikpsychologie".

His study of a musical prodigy (German; 1916, English; 1925) was his first publication in an area, in which he has been continuously active since: the study of the creative personality. Over the years, he devoted a great many papers to this subject. Only recently (1952) his important work on "Talent and Genius" appeared, simultaneously in Dutch and German. Based on a large historical, biographical and psychological material, it constitutes a new approach to an intricate and fascinating problem.

Révész' work in other fields, however, did not detract from his interest in the fundamental problems of perception. His main publications here deal with the sense of touch. Tactile perception, though it is of fundamental importance, has been to a large extent ignored by other perception theorists. Many current theories of perception, although pretending to general validity, are based nearly exclusively on optic and acoustic data. Révész was able to demonstrate conclusively, that tactile perception has many distinctive properties, different from and irreducible to those of visual and auditory perception. Here, as everywhere, Révész has constantly objected to sweeping and premature generalizations (to which, he felt, especially Gestalt psychology was prone). His research in tactile perception reached its summit and conclusion with the publication of the two volumes of his "Die Formenwelt des Tastsinnes" (1938).

This research had brought him into contact with the blind, in whose experience tactile data play so dominant a role. Moved by human sympathy as well as by scientific interest, after the appearance of the "Formenwelt" he continued his studies in the psychology of the blind. His very latest work (Dutch; April 1955) edited in cooperation with several other specialists, bears the title "The personal and social life of the blind".

His aesthetic interests, already apparent in his works on music, did not fail to manifest itself in his studies of the blind. A major part of the "Formenwelt" is devoted to a richly documented treatment of the art of the blind. The revised English edition of this book, published in 1950, is aptly entitled "Psychology and art of the blind".

Révész' research in animal psychology belongs under the heading of comparative psychology. For a long time he was preoccupied with the distinctive differences between humans and animals. In the course of these studies he was confronted recurrently with the problems of language. Although in scientific literature the expression "animal language" is often used, Révész has always taken exception to this terminology, which in his opinion implied a slurring over of crucial distinctions. Many of his papers

are devoted to this subject. Gradually, his thinking came to focus on the problem of the origin of language. After several preliminary attempts, in 1946 he published a large book on "Ursprung und Vorgeschichte der Sprache" (Origin and prehistory of language), containing, next to an incisive criticism of existing theories, based on logical, semantical and psychological considerations, a proposal for a new theory. This theory, constructed upon a linguistic and psychological foundation, sheds new light on an age-old problem, and opens new ways to its solution. It has had already considerable impact upon linguistic thinking, and has led to renewed interest in the problem of the origin of language.

Révész has also been actively interested in various fields of applied psychology, like educational psychology, industrial psychology, psychology of the armed forces. He has always stressed the necessity of an adequate theoretical basis for the application of psychology. Repeatedly he has warned against the dangerous trend in present day psychology to separate applied from theoretical psychology, both in the training of psychologists and in their professional work.

Révész was an exceptionally gifted man. The versatility of his mind is sufficiently attested by the above review of his most important works. And this review is far from complete. The list of his papers runs into the hundreds. The variety of his interests did not lead, as it does in so many others, to superficiality. On the contrary: in each of the different fields he was active in, he was a specialist. All his publications show a profound knowledge of the subject they deal with. His approach was always both original and sound, his contributions represented a real advance in scientific knowledge.

He has had very many pupils. They remember with gratitude his genuine interest in their problems, his helpfulness in difficulties, the warmth of his affection, the richness of his personality. Many of them have become and have remained his close friends.

Révész was one of the most distinguished and most productive representatives of scientific psychology. He has witnessed and furthered its rapid growth since the beginning of this century. His work stands as a hallmark of the best that has been achieved during this period. As such, its value will remain undiminished by the progress of time.

H. C. J. DUIJKER

## INTERDEPENDENCE IN JUDGMENTS OF SPACE, TIME AND MOVEMENT

BY

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Space and time in human experience, while recognised as fundamental, have usually been discussed as separate and autonomous modes of perception. The question of their possible interdependence appears to have been little studied since Helson's (1) original experiment. The importance of this question becomes apparent with the growing recognition of the wide range of inter-modal phenomena in perception and of the basic character of the temporal dimension in experience generally (2, 3).

The idea that the qualities of experience can be identified by an observer in terms of the physical characteristics of the stimulus has arisen because of the supposed correspondence between autonomous physical measures and the constituents of experience. So the subject matter to be so to speak, has been taken to be derivable from its presupposed discrete physical dimensions. The mutual independence of physical measures has been investigated has been predetermined. What happens "inside" experience, been imposed on experience. This procedure may appear to have a certain plausibility in establishing, for instance, the psychic correlates of the physical dimensions of sound waves, although the interdependence of pitch and loudness in contrast to the independence of frequency and intensity is well known (4). The procedure is devoid of any plausibility in studying, for example, judgments of long intervals of time (5).

That interdependence is a primary feature of experience is the conclusion we reach both from phenomenological analysis (6) and developmental studies. The possibility of resolving the complex of experience into distinguishable aspects only arises at a late stage of scientific sophistication (7). One of the psychologist's tasks is to analyse the phenomenal order in terms of its experienced constituents and then to determine their physical correlates. In this way the basic phenomenological datum is preserved whilst measured in physical units. The experiment to be described takes this point of departure.

## AIM OF THE EXPERIMENT

The  $\tau$ -effect identified by Helson and King (1) in 1931 led us in a previous paper (8) to study the effect of spatial relations on judgments of temporal intervals. We demonstrated a phenomenon such that when three flashes of light are successively presented by equal temporal intervals, one temporal interval will seem longer as the relative distance between the corresponding flashes increases. We designated this phenomenon the  $\kappa$ -effect. In the present communication we propose to study the variation in the effect under different conditions.

At the time we reported these experiments we were unaware that a phenomenon of this nature had been described by Japanese investigators (9, 10, 11) employing quite different procedures. Price-Williams (12) has recently carried out independent experiments confirming the effect.

## PROCEDURE

The subject sat in a dark room and observed in front of him a repeated succession of three equal flashes of light set in order along a line either horizontally or vertically. Each cycle of three flashes was repeated after an interval of  $\frac{5}{3}$  of the total cycle. By adjusting a knob he was able to control the timing of the flash of the centre light over a range varying from simultaneous with the first light to simultaneous with the third. That is, if the interval of time between lights 1 and 2 is designated  $t_1$ , and that between lights 2 and 3 designated  $t_2$ , he adjusted the knob until he judged  $t_1$  equal to  $t_2$ . The distances between the flashes of light could be varied by the experimenter. If the first distance is called  $d_1$  and the second  $d_2$  the ratio  $d_1/d_2$  could be varied. The cycle of lights was repeated continuously until the subject had completed his adjustment.

There were several essential differences between this procedure and that described by the Japanese investigators. In the Japanese studies the subject compared single intervals of fixed distance and varying time with a standard. Secondly, their standard and variable presentations were not immediately consecutive in time or space. Thirdly, in their experiments only a single presentation was made for each judgment, the subject reporting equal, greater or less than the standard, in accordance with standard psychophysical procedure.

In the experiment to be reported here, the subject adjusted two temporal intervals for equality. Secondly, the spatial intervals were adjacent along a line so that the temporal intervals to be adjusted were immediately consecutive. Thirdly, the spatial intervals were presented in

a continuous cycle until the subject was satisfied with his adjustment.

In view of our intention to examine variations in  $\alpha$ -effect dependent on order and mode of presentation, it was desirable to determine the optimal conditions for the major variables which were to be kept constant in this experiment namely, distance ratio and visual angle. The distance ratios investigated were such that when one distance varied in unit steps from 1 to 13, the other remained constant at 1. For any visual angle (from  $2^\circ$  to  $40^\circ$ ) the optimal distance ratio is 3/1. At a distance ratio of 3/1 the optimal visual angle was approximately  $7^\circ$ . These values of distance ratio and visual angle were employed throughout the experiment.

#### APPARATUS<sup>1</sup>

Four 6 v. pea-lamps enclosed in tubular holders were arranged so that they could move up and down a metal bar enclosed in a box measuring  $3' \times 5'' \times 6''$ . These holders were fitted with masks and diffusers giving a vertical slit of homogenous light 1 mm.  $\times$  12 mm. The front of the box was fitted with frosted glass, the light apertures being  $\frac{1}{2}''$  behind this. The lamps were connected to a rotary contact device via two change-over switches so that a sequence of three light flashes could be continuously repeated with different combinations of lamps. The time interval between the first and third flashes remained constant at 1.5 seconds, and the timing of the middle flash was continuously variable. Details of the arrangements are shewn in Fig. 1. The circuit is shewn in Fig. 2.

The contact maker was driven by a constant speed motor reduced by gears to  $\frac{1}{4}$  R.P.S. The resulting speed constancy permitted a time scale to be calibrated directly by subdivision of the arc subtended.

The contacts were arranged as follows:  $c_1$  and  $c_2$  were permanently fixed and arranged on the circumference of a circle. The time taken for the contact to move from  $c_1$  to  $c_2$  determined the length of the time scale. For greater reading accuracy the pointer of the scale was arranged to move twice the angle by means of gears. The position of  $c_3$  was adjusted by the subject via a 6 ft. flexible drive and could be placed anywhere within the  $360^\circ$  of the contact makers' traverse. This adjustment could also be effected by the experimenter for resetting at the beginning of each trial. The control knob for this and all switches were incorporated in the same box for ease of manipulation by the experimenter.

<sup>1</sup> We are indebted to Mr. R. Church for designing and constructing the apparatus employed in this research.



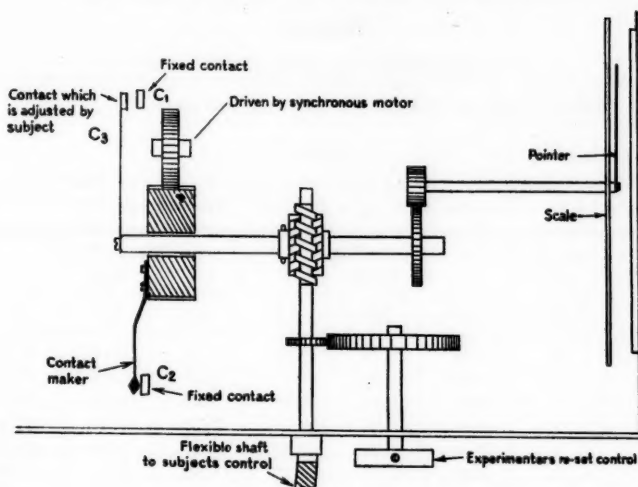


Fig. 1

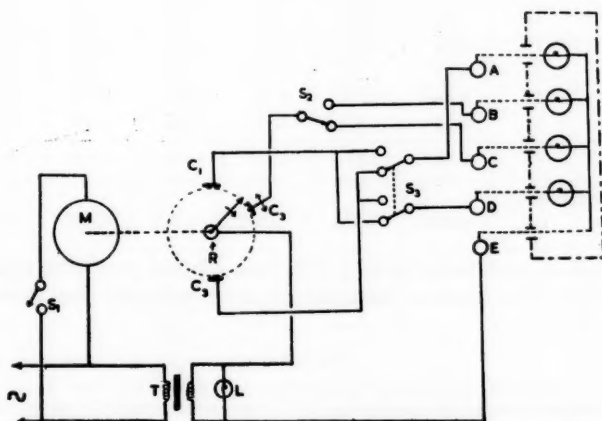


Fig. 2

Terminals A-E used to connect display unit via 5-way cable.  
 M is constant speed motor which drives the contact-maker R.  
 T supplies power for the scale illuminator L and display unit.

## RESULTS

## I. Presentation of Unequal Distances

In Tables 1 to 6 we present the experimental data.

TABLE 1

Mean Judgments of  $t_1$  (secs.) under conditions of horizontal presentation  
 $T = (t_1 + t_2) = 1.5$  secs.  $D = (d_1 + d_2) = 18''$ . Visual angle =  $7^\circ$ .  $N = 15$

Sub- ject	No. of judg- ments	Left to Right $d_2/d_1$			Right to Left $d_2/d_1$		
		1/3	3/1	1/1	1/3	3/1	1/1
A	9	0.68	0.79	0.70	0.76	0.78	0.73
B	9	0.69	0.78	0.77	0.73	0.76	0.74
C	9	0.70	0.77	0.72	0.66	0.77	0.72
D	6	0.69	0.76	0.71	0.71	0.74	0.75
E	4	0.69	0.76	0.65	0.64	0.73	0.71
F	4	0.66	0.76	0.79	0.66	0.79	0.72
G	4	0.48	1.11	0.64	0.64	0.98	0.66
H	4	0.57	0.75	0.67	0.69	0.75	0.74
I	4	0.71	0.74	0.72	0.62	0.85	0.67
J	4	0.75	0.77	0.81	0.66	0.76	0.81
K	1	0.71	0.82	0.74	0.69	0.82	0.67
L	1	0.74	0.79	0.80	0.71	0.79	0.79
M	1	0.67	0.94	0.73	0.72	0.80	0.66
N	1	0.66	0.73	0.72	0.64	0.79	0.73
O	1	0.75	0.82	0.77	0.76	0.89	0.78
Means . . . .		0.68	0.81	0.73	0.69	0.80	0.73
Standard devia- tions . . . .		0.068	0.095	0.050	0.043	0.063	0.045

It will be seen from Table 1 that three subjects gave nine repeated judgments. The standard deviations of these judgments are shown in Table 2.

TABLE 2

Standard Deviations of Judgments of Subjects 1, 2 and 3

Subject	Left to Right $d_2/d_1$			Right to Left $d_2/d_1$		
	1/3	3/1	1/1	1/3	3/1	1/1
A . . . . .	0.1082	0.0840	0.0815	0.0938	0.0596	0.0537
B . . . . .	0.0680	0.0821	0.0930	0.0946	0.0605	0.0394
C . . . . .	0.0650	0.1672	0.1630	0.1020	0.1333	0.1063

TABLE 3  
Probability values from *t*-test for means in Table 1

	Left to Right			Right to Left		
	1/3	3/1	1/1	1/3	3/1	1/1
Left 1/3 to 3/1 Right 1/1	< 0.01 < 0.05					
Right 1/3 to 3/1 Left 1/1	> 0.5 < 0.01 < 0.02	< 0.01 > 0.5 < 0.01	< 0.05 < 0.02 > 0.5	< 0.01 < 0.02		

TABLE 4  
Individual judgments of  $t_1$  (secs.) under conditions of vertical presentation  
 $T = (t_1 + t_2) = 1.5$  secs.  $D = (d_1 + d_2) = 18''$ . Visual angle =  $7^\circ$ .  $N = 15$

Subject	UP - lower light first $d_2/d_1$			DOWN - upper light first $d_2/d_1$		
	1/3	3/1	1/1	1/3	3/1	1/1
B . . . . .	0.85	0.80	0.73	0.61	0.63	0.73
C . . . . .	0.74	0.67	0.65	0.69	0.67	0.76
D . . . . .	0.72	0.75	0.72	0.72	0.79	0.75
E . . . . .	0.79	0.77	0.75	0.64	0.89	0.81
F . . . . .	0.69	0.70	0.70	0.80	0.83	0.60
G . . . . .	0.55	0.86	0.73	0.33	1.12	0.77
H . . . . .	0.73	0.81	0.75	0.63	0.87	0.65
I . . . . .	0.61	0.77	0.81	0.69	0.80	0.89
P . . . . .	0.77	0.74	0.75	0.70	0.82	0.71
Q . . . . .	0.77	0.63	0.65	0.60	0.99	0.69
R . . . . .	0.65	0.76	0.73	0.73	0.79	0.75
S . . . . .	0.77	0.67	0.85	0.71	0.76	0.77
T . . . . .	0.72	0.91	0.71	0.62	0.91	0.71
U . . . . .	0.66	0.71	0.73	0.65	0.67	0.69
V . . . . .	0.70	0.81	0.85	0.60	0.75	0.67
Means . . . . .	0.71	0.76	0.74	0.65	0.82	0.73
Standard deviations . . . . .	0.073	0.073	0.057	0.101	0.123	0.073

TABLE 5  
Probability values from *t*-test for means in Table 4

		UP			DOWN		
		1/3	3/1	1/1	1/3	3/1	1/1
UP	1/3						
	3/1	< 0.1					
	1/1	< 0.5	< 0.5				
DOWN	1/3	< 0.01	< 0.01	< 0.02			
	3/1	< 0.01	< 0.5	< 0.01	< 0.01		
	1/1	< 0.5	< 0.5	> 0.5	< 0.05	< 0.05	

TABLE 6  
Mean judgments of  $t_1$  and  $t_2$  in seconds under different conditions of presentation

$d_1$	$d_2$	Position of $d_1$							
		Left →		Right ←		Above ↓		Below ↑	
		$t_1$	$t_2$	$t_1$	$t_2$	$t_1$	$t_2$	$t_1$	$t_2$
small	large	0.81	0.69	0.80	0.70	0.82	0.68	0.76	0.74
large	small	0.68	0.82	0.69	0.81	0.65	0.85	0.71	0.79

The data in Table 6 have been analysed to show the following five effects. The letter *t* invariably refers to values in seconds.

1. *Effect of size of distance on the values of  $t$  allotted to it ( $\alpha$ -effect).* This is given by the difference between the mean values of  $t$  for the larger and smaller distances respectively, regardless of the spatial orientation of the distances or the serial order of their presentation.

Mean values of  $t$ :

$d = 1$ , 0.8075;  $d = 3$ , 0.6925. C.R. = 10.2,  $p < 0.0001$ .

It is clear that the smaller distance is allotted a larger interval of time.

2. *Serial order effect.* This is given by the difference between the means of mean values for  $t_1$  and  $t_2$  regardless of size or orientation of the distances. Each mean value is obtained from combined values for the larger and smaller distances. To test the significance of the difference, the  $\alpha$ -effect has been eliminated by taking these values in the pairs for each orientation (see figures in effect 4, *infra*).

Mean values:  $t_1 = 0.74$ ,  $t_2 = 0.76$ . The difference is very highly significant.

The difference between these two means indicates that when a subject adjusts the two successive intervals for equality he tends to make the second larger than the first.

3. *Effect on  $t_1$  and  $t_2$  of time order of presentation of distances.* This is given by the difference between the mean values of  $t_1$  and  $t_2$  (a) when the larger distance is presented first and second respectively and (b) when the smaller distance is presented first and second respectively. These values are obtained regardless of the spatial orientation of the distances.

(a) Mean values when larger distance is presented first:

$t_1 = 0.6825$ ;  $t_2 = 0.8175$  C.R. = 10.8,  $p < 0.0001$

(b) Mean values when larger distance is presented second:

$t_1 = 0.7975$ ;  $t_2 = 0.7025$  C.R. = 5.5,  $p < 0.0001$ .

4. *Effect on  $t_1$  and  $t_2$  of position of  $d_2$  in relation to  $d_1$ .* This is given by the difference between the mean values of  $t_1$  and  $t_2$  under conditions when  $d_1$  is presented to the left, right, above or below  $d_2$ , regardless of the size of  $d_1$  and  $d_2$ .

Mean values when first distance is presented:

to the left,  $t_1 = 0.745$ ;  $t_2 = 0.755$

to the right,  $t_1 = 0.745$ ;  $t_2 = 0.755$

above,  $t_1 = 0.735$ ;  $t_2 = 0.765$

below,  $t_1 = 0.735$ ;  $t_2 = 0.765$

None of the differences between these pairs of means is significant.

5. *Effect on the mean values of  $t$  for the smaller and larger distances with variation in their position and time order of presentation.* This is given in Table 7 by the differences between the mean values of  $t$  when the first distance is to the left, right, above or below the second.

TABLE 7

*Mean value of  $t$  in seconds under different conditions of presentation*

Size of distance ( $d$ )	Position of $d_1$			
	Left →	Right ←	Above ↓	Below ↑
smaller . . . . .	0.815	0.805	0.835	0.775
larger. . . . .	0.685	0.695	0.665	0.725
difference . . . . .	0.130	0.110	0.170	0.050

The essential difference between effects 4 and 5 is that in 4 we are concerned with the way the location of the second distance in relation to the first may influence the serial order effect. In effect 5 we are concerned with the influence on  $\kappa$  (serial order eliminated) of the location of the second distance in relation to the first. This effect cannot be derived from the combined influence of  $\kappa$  and effect 4.

## II. Presentation of Equal Distances

In the previous section we have shown the effects on temporal judgments of presenting unequal distances. In certain cases it is possible to eliminate statistically the influence of inequality of distance. As an experimental control we show in Table 8 the effects on temporal judgments obtained when equal distances are in fact presented.

TABLE 8

*Mean values of  $t_1$  and  $t_2$  in seconds, under different conditions of presentation, when  $d_1 = d_2$*

Position of $d_1$							
Left →		Right ←		Above ↓		Below ↑	
$t_1$	$t_2$	$t_1$	$t_2$	$t_1$	$t_2$	$t_1$	$t_2$
0.73	0.77	0.73	0.77	0.73	0.77	0.74	0.76

The data in the above table has been analysed to show two effects. The  $\kappa$ -effect and influence of order or presentation of distances (effects 1 and 3 above) do not arise here as we are dealing with equal distances. The two effects are:

### *Serial order effect*

Mean values:  $t_1 = 0.7325$   $t_2 = 0.7675$ .

These figures do not differ significantly from the values of  $t_1$  and  $t_2$  obtained when unequal distances are presented.

### *Effect on $t$ of the position of one distance in relation to the other*

$d$  at left, 0.75 secs;  $d$  at right, 0.75 secs;

$d$  above, 0.745 secs;  $d$  below, 0.755 secs.

None of the differences between these means is significant.

## DISCUSSION OF RESULTS

Our experimental results show that under a variety of experimental conditions temporal judgments about events which have spatial and temporal components are affected by the spatial component. The subject allots a shorter time interval to the larger distance because a time interval physically equal to that allotted to the shorter distance would seem to him longer. The  $\kappa$ -effect is thus the counterpart of the  $\tau$ -effect which demonstrates the influence of the temporal component on spatial judgments. It would seem therefore from the two phenomena ( $\tau$  and  $\kappa$ ) that the spatial and temporal components of the space-time events about which judgments are made are psychologically interdependent.

Turning to effects 2 and 3 in the presentation of unequal distances we should note that a particular feature of our experimental procedure is that the subject divides a total interval of time into two durations so that they appear to him equal. The total distance, corresponding to the total time interval, is divided by the experimenter into two unequal parts either of which may be presented first. The order in which the experimenter presents the larger and smaller distances may conceivably influence the temporal judgments. Accordingly, if we wish to estimate any possible effect due to the order of presentation of larger and smaller distances we must first study any serial order effect (i.e. effect 2) which normally occurs in experiments such as these.

The effect of serial order in these experiments is that the subject tends to make the second temporal interval greater than the first. This in itself produces different values of temporal judgments for either the larger and smaller distances according to the order in which it is presented. When the larger distance ( $d = 3$ ) is presented first its value (from effect 1 corrected for effect 2) may be expected to be:

$0.6925 \times 0.74/0.75$  secs., and when presented second,

$$[0.6925 \times 0.76/0.75 \text{ secs}$$

where  $T = 1.5$  secs., and  $T/2 = 0.75$  secs.

Effect 3 may thus be compared with its value as calculated from effects 1 and 2 as set out on the next page.

Thus after allowing for the  $\kappa$ -effect and the effect due to the serial order there is no appreciable residual effect due to the order in which the distances are presented.

	Order of presentation	Measured (effect 3) secs.	Calculated (from effects 1 and 2) secs.	Difference
$d = 3$	first	0.6822	0.6835	+ 0.0013
	second	0.7025	0.7015	- 0.0010
$d = 1$	first	0.8175	0.8180	+ 0.005
	second	0.7975	0.7970	- 0.005

#### THEORETICAL IMPLICATIONS

In attempting to account for the  $\alpha$ -phenomenon we must direct our attention to psychological types of explanation. We can immediately rule out such considerations as retinal reaction time in view of the fact that the phenomenon occurs in time intervals when such factors would be negligible.

We are inclined to favour an explanation in terms of the experience of movement. In everyday life experience is formed in relation to permanent objects which preserve their identity while moving from one place to another (13) in spite of the fact that visual cues may be intermittent. Thus an object moving along a partially obscured path may become visible at three or more points. In common experience this is perceived as a single enduring moving object, not as different objects each enjoying a transient existence. In the laboratory these transitory appearances can be created by means of flashing lights, and what the observer perceives is virtually the same as what he would have perceived had the object been a constant one. The conditions of our experiment are such that they would produce cues indistinguishable from those arising from objects moving along a partially obscured path. Since in the subject's past experience such cues have arisen in connexion with moving objects they now tend to be seen as moving objects (14). The following considerations support such an explanation:

(a) The display resembles the common everyday experience of intermittently seeing a moving object at several discrete points in a path which is partially obscured. Subjects in fact tend to refer to the phenomenon in these terms.

(b) Brown's (15, 16) experiments with moving objects yield results which are sufficiently similar to ours to justify this interpretation. More specifically there is similarity in the two sets of results in the extent of the effect of distance on temporal judgment; in a decline in the effect at



distance ratios greater than 1/4, 4/1; in the fact that the effect appears to be uninfluenced by subjects' attempts to judge the passage of time by counting; and in respect of the direction in which the lights appear.

(c) Our familiarity with the acceleration of falling objects and the deceleration of rising objects might lead one to expect apparent acceleration downwards, deceleration upwards and more linear movement horizontally in our display. Effect 5 which shows variation in  $\kappa$  with variations in the upwards, downwards and horizontal directions and the data shown in Fig. 3 support this interpretation.

We now turn to effect 4. In examining any possible influence of the position in which distances are presented it should be clear that there are two cases. Consider any distance about which a temporal judgment is being made: (a) this distance may occupy a position to the left, right, above or below another distance, regardless of whether it is presented first or second, (b) a distance occupied by one of these positions (e.g. "above") may be presented either first or second in a time sequence. The influence of (a) has been examined as effect 4 *supra*. and shown not to be statistically significant. The influence of (b) has been examined under effect 5 *supra*. even after allowing for  $\kappa$  and serial order effects and shown to be significant. Case (b) is distinguished from (a) in involving the factor of direction. The three flashes of light appearing in sequence along a line may be considered as marking out a direction such that if a fourth light should appear its position would be psychologically pre-determined.

It is this factor of direction which appears to determine the magnitude of the  $\kappa$ -effect. It is a factor which cannot be derived from any combination of other effects. If the third light appears to the right (left) of the first light, the direction is said to be to the right (left). The diagram below shows that the effect is smallest in the upward, greatest in the downward and intermediate in the horizontal direction. The arrows show the directions indicated by the order of appearance of the lights.

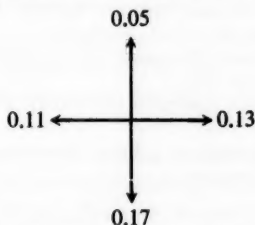


Fig. 3. Magnitude of  $\kappa$ -effect with variation in direction.

Thus the factor of direction in the present context must be interpreted as direction relative to the earth.

To conclude, the phenomena appear to support the primacy of discriminations of movement over separate spatial or temporal discriminations. The primacy of movement is indeed biologically vital and almost certainly characterises individual development. Even at the late stage of scientific sophistication when the spatial and temporal aspects of events are distinguishable in experience their interdependence remains. Interdependence cannot be predicted from a knowledge of separate physical dimensions, but must be examined in its own right. It follows that the study of interdependence is necessary to provide the full picture of mental development.

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# THE RELATIONSHIP BETWEEN SECONDARY FUNCTION AND SOME ASPECTS OF SPEED AND TEMPO OF BEHAVIOUR

BY

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The concept of secondary function, first put forward by Gross (10) to account for characteristic differences between the states of mania and depression, and thereafter developed by Wiersma (27), Heymans (11), Heymans and Wiersma (12) and Heymans (13) as a dimension of personality, is usually considered to be synonymous with perseveration.

Justification for the assumed equivalence of the two concepts can presumably be found in Wiersma's work on critical fusion frequency and sensory adaptation, and in Heymans's statement that people can be called primary or secondary functioning according to the degree in which cognitive and affective processes *perseverate*, in proportion to their importance. The word actually used in the Dutch text is "nawerken", equivalent to the German "nachwirken" (Heymans, 13, p. 190). A more careful reading of the original texts shows, however, that although Heymans had some undefined neural inertia process in mind as a determinant of primary-secondary function, he believed its behavioural manifestations to be diverse and to include, besides cognitive and affective perseveration, such temperament attributes as stimulability and mobility of attention, general reactivity, speed and evenness of psychic tempo and variability of mood. Experimental work on perseveration outside the Dutch school confined itself from the beginning to its narrower manifestations in the form of "lag", though it may be remembered that Lankes (15) included in his study, in addition to hindrance and flicker tests, such tasks as cancellation and finger tapping at natural rate.

In later investigations, motor perseveration tests involving hindrance effects were gradually replaced by creative effort tests in which the will is opposed to inherent disposition rigidity. To Cattell (8, p. 440) the latter has become the central feature of perseveration, which thereby acquires character implications foreshadowed by other investigators (Pinard, 17, Stephenson, 20). Cattell does not attempt to explain the connection

between disposition rigidity and mental inertia. The two are merely treated as major and minor factors in the same domain (Cattell, 8, pp. 433—442). Some writers hold that apart from disposition rigidity, there is no evidence for the existence of an additional "pure" *p* factor at all (Walker, Staines and Kenna, 26).

A concurrent line of research was pursued by a small group of workers, who examined Heymans's original hypothesis and confirmed many of his findings by means of questionnaire and experimental studies (Biesheuvel, 4, 5, 6). In the course of factor analysis of various types of rating scale it was found that aspects of the inertia, stimulability, mobility and variability of behaviour could be reduced to secondary function as a unitary factor (Baehr, 2, 3). It follows that Cattell is not justified in replacing the primary-secondary function terminology by perseverance—non-perseveration, as he does in describing the Heymans-Wiersma theory (Cattell, 8, p. 22). Mental inertia, once their common foundation, has virtually disappeared from the definition of perseverance, whilst it has been retained to define secondary function. To end the present state of confusion, it may be advisable to drop the term perseverance as referring to a dimension of temperament; to replace it by disposition rigidity in so far as certain aspects of character are concerned; and to retain secondary function as a temperament concept which includes perseverance, in the sense of "lag" or "hindrance effects", as part of its behavioural syndrome.

The behavioural implications of secondary function need, however, to be further examined, preferably by means of objective criteria and concrete test performances of the kind that were used in the study of perseverance.

Some experimental work using objective tests, other than the familiar *p* or flicker fusion tests, was carried out by the Aptitude Tests Section of the South African Air Force during World War II. A multiple correlation was obtained between a secondary function rating based on observational data and certain aspects of psychomotor test performance, namely speed and variability scores derived from a psychomotor learning test; ratio between time and errors score obtained from a steadiness of movement test and ratio of time taken for the first and the mean of the next four trials on a two-hand coordination test. This correlation was .74, when only the extremes of the s.f.—p.f. distribution were used ( $n = 118$ ) and .40 in an entirely different sample with a normal distribution ranging from extreme primary to extreme secondary function ( $n = 398$ ). (Biesheuvel, 7).

This paper is concerned with a further experimental investigation of the relationship between secondary function and speed and tempo aspects of behaviour.

## EXPERIMENTAL DESIGN

A number of tests was selected in which performance depended either on habitual rate of expressive movement, or on speed of response, restricted by psychomotor, perceptual or intellectual factors. A ranking of subjects in order of primariness, obtained by means of paired comparisons, was used as a criterion of secondary function. This simple method was chosen because the observational assessment method, apart from its time consuming nature, would have introduced a spurious element, speed of work in a variety of psychomotor tests being one of the criteria on which it relies to assess strength of secondary function. It was thought that most valid results would be obtained by using as assessors, professional members of the N.I.P.R. staff with a thorough appreciation of the behavioural implications of secondary function. As subjects were chosen professional, clerical and administrative staff members, with whom the raters were more or less equally well acquainted. This inevitably limited the experimental sample to little more than 50 cases; but the smallness of this population is offset by the greater validity of assessment which can be expected when people are constantly in contact with one another in the work situation and in the staff room. There were twenty-five assessors, all of whom also acted as subjects, so that a number of self-ratings was included. Only two of the assessors had knowledge of the purpose of the experiment.

From the correlations between tests and criterion, it should be possible to infer what aspects of speed, if any, can justifiably be looked upon as part of the secondary function behaviour pattern. A multiple correlation should give some indication of the extent to which secondary function assessments may be predicted by means of objective test scores.

The unknown validity of the criterion is a serious limitation in this experimental plan. If the assessors thought of primary functioning people as predominantly quick in movement and gesture, and if they overweighted these attributes, which are rather more readily observable in everyday situations than certain other manifestations of primary function, then an unduly large relationship between the criterion and tests designed to measure "quickness" would be bound to ensue. Because of the possibility of occurrence of this error, a relationship between the tests and the criterion could only then be considered as proof of a connection between behavioural tempo and secondary function if there were positive indications to this effect in the pattern of test inter-correlations.

To allow for this, the tests were chosen in such a way as to include some which, as far as introspection could tell, were largely a matter of

motor speed, whilst in the case of others, performance also depended on flexibility and freedom from inertia. Examples of the latter are Repeated Letters, which would seem to involve the ability to interrupt and resume a visual scan with a minimum of hindrance, and Pursuit in which performance appears to depend on the ability to achieve perceptual closure. By factoring the inter-correlations between the criterion and the tests chosen in this manner, a more compelling interpretation of the relationships obtaining between them might be obtained.

Another experimental approach which seemed likely to assist interpretation was that pursued by Mundy-Castle who in an attempt to find a neurophysiological basis for the concept of secondary function obtained reasonable evidence to support the hypothesis that alpha frequency in the electroencephalogram and secondary function are related. Further investigation also showed alpha frequency to be related to performance on a number of the speed tests, chosen for this investigation.<sup>1</sup>

It was therefore decided to use frequency of alpha rhythm as a measure providing information about central neural function and to include it as a test in the factor experiment. To check the possibility that *g* might be responsible for some inter-correlation between the tests, as many subjects as possible were given the advanced version of the Progressive Matrices test (as used for officer selection in the Services).

#### DESCRIPTION OF TESTS AND TEST PROCEDURES

##### 1. *Tapping Rate*

This was measured by means of apparatus consisting of a tapping key and counter. Subjects were instructed to tap as fast as possible for a period of 30 seconds. This gave their "tapping fast" score. Thereafter they were given three 30 second trials in which they could tap at their natural rate, a speed which they felt to be comfortable and to which they would spontaneously settle over a long period. The mean number of taps per half minute gave the "tapping, own speed" score. In the analysis of scores, these two tapping scores are treated as separate and will be referred to as Tapping I and Tapping II respectively.

##### 2. *Rate of Drawing Crosses*

The task was to draw crosses (x) within the squares on  $\frac{1}{4}$  inch squared paper, first for one minute as fast as possible, thereafter for three one-minute periods at a preferred rate, and thereafter again as fast as possible

<sup>1</sup> Details of this and other investigations on the relationship between alpha frequency and secondary function are reported in Mundy-Castle's paper in this issue.

for one minute. There was so little difference between the numbers drawn at the preferred and speeded rates that only the former was used, the score being the mean number of crosses drawn per minute.

### 3. *Walking Speed*

In order to obtain a measure of natural walking rate, it was important that subjects should not realise that this was being measured. Each subject was therefore instructed to walk from the laboratory to a point in the grounds about 500 yards away where a member of the laboratory staff further instructed him to observe a corner of an adjoining agricultural showgrounds for a period of 30 seconds and to return to the laboratory by the same route where he was to write down what he had observed in detail. The vantage point was visible from the laboratory, and by means of a system of signals, the time taken to walk from a particular point outside the laboratory to the vantage point and back could be taken. This total time was treated as a measure of natural walking rate. The subject could be seen at any point of the route by either of two observers, stationed at each end, and any irregularities, such as running or stopping, could be noted. It is not known to what extent the real purpose of this test was concealed by this procedure. It is probable that a number of subjects suspected that the observational report was not the only objective. No correction was made for leg length, as a result of which the scores only partly reflect the rate at which the legs were moved.

### 4. *Talking Speed*

Each subject was called in turn to the office of the second author and engaged in conversation with two or three other members of staff about casual subjects. The candidates knew that the conversation was related to a test, and that it was being recorded, but no one knew the precise purpose of the test, nor at what stage the wire recorder was switched on. Whether this caused some subjects to be more deliberate and therefore slower in speech it is impossible to say. By playing back the record, the length of the various remarks made by a subject in the course of the conversation could be measured in seconds, after which the words themselves were counted. Talking rate was calculated in terms of number of words per minute.

### 5. *Writing Speed*

Subjects were given a passage from a newspaper advertisement, which had been typed out on a test sheet. They were instructed to copy this in



their own handwriting, with punctuation. There was no suggestion that this should be done either fast or slowly. The passage was chosen in such a way that speed of copying was unlikely to be influenced by strangeness or familiarity of its content.

#### 6. *Adding Speed*

Subjects were required to add single digits, arranged in columns of 10 rows. There were 21 columns, the score being the time taken by the subject to complete them all, regardless of errors.

#### 7. *Sorting*

A set of 80 brass discs, engraved with one or two capital letters or one or two digits had to be sorted from a tray into a box with 16 compartments, each corresponding to a particular symbol or combination of symbols as engraved on the discs. The score was the time taken to complete this task, regardless of errors.

#### 8. *Formboard*

Apparatus consisted of a laminated, metal-faced board, into which twenty spaces were cut with from three to six sides. Twenty pieces, to be fitted into these spaces, were laid out on a presentation board. Each piece fitted into one hole only. The score was the time taken to complete the task.

#### 9. *Pursuit*

Thurstone's adaptation of MacQuarrie's Pursuit Test was used, in which ten lines weave from the left to the right of a rectangular area, crossing many times on their way. In our use of this test, subjects were required to follow each line with a pencil and enter its number at the terminal space at the right. Eight blocks of ten lines were given, the score being the time taken to complete the task regardless of errors.

#### 10. *Repeated Letters*

On sheets of printed letters (30 letters per line, 20 lines per page) subjects were instructed to ring every instance where a letter was the same as its predecessor. Repeated letters occurred in doublets or triplets, from 16 to 25 times per page. A number of misleading combinations also occurred, such as pqp, nm, etc. Subjects were required to work through eight pages, their score being the time taken to complete this task, regardless of errors. A short preliminary exercise was given.



### 11. *Identical Pictures*

The test consisted of rows of seven pictures, one of which was identical to an eighth picture given in the left hand margin, all the others being slightly different. The subject's task was to find the identical picture and underline it. There were four pages of pictures, each with fifteen rows. The score was the time taken to complete this task, regardless of errors. The test was preceded by a short exercise.

Except in the case of Tapping I and Crosses (fast) no emphasis was laid on speed of work. The subjects were merely instructed to carry out the task in their own time. In tests 6, 7, 9, 10 and 11 where accuracy imposes a greater or lesser restriction on speed, the subjects were left to decide for themselves which should be given the greater weight. We assumed that this would be done less as a matter of voluntary decision than of habitual mode of response. As some of the tests were administered to small groups of subjects working together, a competitive element did intrude in a number of cases, causing them to depart from their natural or habitual tempo. It will be seen that we proceed in these tests from simple motor responses limited only by physiological factors (Tapping I) to responses in which first habit (walking, adding), then simple perceptual discrimination (writing, sorting) act as restrictions, until eventually we are dealing with performances determined largely by perceptual speed and in which motor responses play an increasingly insignificant part. There is thus a shift in emphasis from effector to receptor functions with continuously greater involvement of central processes.

#### TREATMENT OF PRIMARY-SECONDARY FUNCTION RATINGS

Fifty-one members of the N.I.P.R. staff served as subjects for the construction of a criterion by means of the paired comparisons method. The pairs of names, presented in random order, numbered 1283, eight pairs being duplicated to test the consistency of the raters. A total number of 25 professional members of staff acted as raters, their instructions being to underline that member of a pair whom they considered to be the more primary. It was decided to rate in terms of primary rather than of secondary function as a negative correlation was expected between secondary function and speed of work. (In tests which yielded a time score, consistency with this procedure was maintained by ranking scores from low to high). Raters were instructed to omit a pair if they did not know either member sufficiently well to enable them to make a comparison. As a result an unequal number of ratings was obtained for each subject.

The following steps were taken to obtain a primary function rating from this material:

- (a) The frequency with which a subject was rated the more primary member of a pair was obtained for each assessor rating that particular subject.
- (b) This frequency was stated in percentage form and the mean percentage frequency was calculated for each subject.
- (c) As some assessors were likely to be more consistent than others, each assessor's mean deviation from the mean percentage frequencies was calculated.
- (d) The mean and standard deviation of the distribution of these mean deviations was obtained. It was thereupon arbitrarily decided to eliminate all assessors who were placed more than a half S.D. above the mean of this distribution. Five assessors were involved, who turned out to be either comparatively new members of staff, or officers whose work was mainly in the field and who were therefore not as well acquainted with the majority of their colleagues.
- (e) The mean percentage frequencies were thereafter recalculated, the ratings of the unreliable assessors being omitted. This gave the final primary function rating for each subject. It should be clear that no experimental subject was omitted. The effect of the procedure was to reduce the number of raters. In no case was the final rating based on data from more than 20 or less than 7 assessors.

#### ALPHA FREQUENCY

In a separate paper in this issue, Mundy-Castle gives details of the method of recording alpha frequency. He found a correlation of .464 between this phenomenon and primary-secondary function. His subjects were the same as those who did our speed tests, the primary function rating being used as a criterion for both investigations.

#### NORMALITY OF DISTRIBUTIONS

The distributions of both tests and ratings were tested for normality by means of Pearson's skewness coefficient and Geary's ratio (Geary and Pearson, 9). The results are given in Table I. Using the very lenient 1% level, it will be seen that in the case of the criterion, normality is not rejected, though the distribution is rather platykurtic (normality rejected

TABLE I  
*Normality of distributions of test scores and ratings*

Test	Skewness	Kurtosis	n	Rejection of Normality at 1 % level
Tapping I . . . . .	.382	.755	36	Not rejected
Tapping II. . . . .	-.139	.804	36	Not rejected
Crosses . . . . .	-.699	.733	47	Not rejected
Walking . . . . .	.119	.762	46	Not rejected
Talking . . . . .	-.074	.864	41	Not rejected
Writing . . . . .	.732	.827	45	Not rejected
Adding . . . . .	1.330	.735	42	Rejected
Sorting . . . . .	1.003	.687	45	Rejected
Formboard . . . . .	1.669	.714	46	Rejected
Pursuit . . . . .	1.521	.711	47	Rejected
Repeated Letters . . . . .	1.603	.706	48	Rejected
Identical Pictures . . . . .	2.342	.588	45	Rejected
Ratings . . . . .	.049	.862	51	Not rejected

at a 5% level). In the case of six of the tests, namely Adding, Sorting, Pursuit, Repeated Letters, Identical Pictures, and Formboard, normality is definitely rejected. It is not rejected at the strict 5% level in the case of Tapping I, Tapping II and Walking, whilst the distributions of the remaining tests are rather borderline. The abnormality of so many of the distributions places us in a quandary as Kendall's rank correlation coefficient  $\tau$  which is most applicable to this kind of material cannot be used for the kind of analysis we have in mind.

Fortunately, the calculation of a multiple correlation coefficient is not dependent on normality of the underlying bivariate distributions. Though this can be proved mathematically, it will suffice, for the purpose of this paper, to apply the empirical test of correlating the combined weighted score on the tests (obtained by summing the products of the raw scores and their regression weights) with the criterion scores, as a check on the original multiple  $R$ .

We do not know to what extent factor analysis will be affected by non-normality of the correlation surfaces. If errors are introduced, these are more likely to disturb the hypothesis that we are trying to verify, than to confirm it; for it is unusual for a statistical artifact to correspond exactly to a psychological relationship between phenomena, previously established by other means. However this may be, because of the smallness of the experimental samples, it would in any case be imprudent to treat

the factors emerging from this analysis as anything more than trends, helpful in the interpretation of observed relationships and possibly suggestive of further lines of investigation.

#### TEST RELIABILITY

Subjects were retested after an interval of from 21 to 30 days on Writing, Adding, Sorting, Formboard, Pursuit, Repeated Letters and Identical Pictures, the product-moment reliability coefficients being respectively .57; .91; .66; .29; .56; .79; .81. In the case of Tapping II and Crosses, where more than one trial was given, reliability was determined by intercorrelating trials, the median coefficients, corrected by the Spearman-Brown formula being respectively .94 and .98. For Walking, the times for the outward and return journeys were correlated, the result being  $r = .51$ . The functions measured by the tests appear to possess sufficient stability to warrant further investigation, except in the case of the Formboard test, which was therefore eliminated from further analysis. Speed of Talking was included without a reliability check, as none could be devised, whilst Tapping I was included without re-test data on the strength of the high reliability of Tapping II. No re-test of alpha frequency was carried out, as it is generally accepted as being stable.

#### INTERCORRELATIONS BETWEEN TESTS AND CRITERION

The intercorrelations between tests and primary-secondary function ratings are given in Table II.

By means of pivotal condensation, a multiple correlation between the criterion and eleven speed tests was calculated. This gave a value of  $R = .606$ ,  $S.E. = \pm .091$ . Tapping I and Repeated Letters alone give a multiple correlation of .469, which is raised to .519 by the inclusion of Crosses and Pursuit. This leaves an appreciable increment to be contributed by other tests in the battery and points to the fairly general influence of temperamental speed factors on behaviour. The procedure of checking  $R$  by calculating the correlation between the criterion and the combined weighted scores could not be immediately undertaken because of the uneven numbers which completed the various tests. By using only the four most significant tests (Repeated Letters, Pursuit, Crosses and Tapping I) a group of 45 cases could be found of whom 34 had done all tests and 11 had missed out only Tapping I. Regression weights obtained from pivotal condensation for four-test and three-test batteries respectively were used. A correlation of  $r = .488$  was obtained between the criterion and the weighted scores obtained in this way, as compared with the

TABLE II

*Product-moment intercorrelations of tests and criterion*

← (Standard Errors are given below the diagonal)

	Tests	1	2	3	4	5	6	7
1	Tapping I . . . . .	—	.317	.387	.023	-.287	.219	.052
2	Tapping II . . . . .	.148	—	.206	.211	-.272	.116	.218
3	Crosses . . . . .	.146	.164	—	.154	-.086	.600	.423
4	Walking . . . . .	.130	.169	.147	—	.140	.115	.294
5	Talking . . . . .	.160	.161	.159	.157	—	.100	.347
6	Writing . . . . .	.163	.169	.095	.161	.158	—	.503
7	Adding . . . . .	.171	.163	.128	.146	.141	.117	—
8	Sorting . . . . .	.170	.170	.134	.154	.159	.142	.153
9	Pursuit . . . . .	.169	.169	.102	.150	.151	.116	.145
10	Repeated Letters . .	.171	.170	.118	.147	.154	.129	.152
11	Identical Pictures . .	.170	.169	.135	.148	.147	.131	.138
12	P.F.-S.F. Ratings . .	.153	.169	.123	.148	.158	.145	.153
13	Alpha Frequency . .	.151	.154	.159	.146	.170	.161	.167
14	Progressive Matrices.	.208	.201	.146	.170	.155	.169	.175

	Tests	8	9	10	11	12	13	14
1	Tapping I . . . . .	.087	.116	-.028	.158	.310	.402	-.001
2	Tapping II . . . . .	.097	.125	.105	.173	.005	.381	-.187
3	Crosses . . . . .	.329	.567	.444	.344	.404	.149	.282
4	Walking . . . . .	.037	.132	-.107	.198	-.079	-.333	.258
5	Talking . . . . .	.090	.239	.170	.285	.030	.091	.443
6	Writing . . . . .	.240	.479	.371	.379	.154	.146	.269
7	Adding . . . . .	.142	.288	.156	.374	.151	-.099	.274
8	Sorting . . . . .	—	.102	.331	.396	.029	-.032	-.007
9	Pursuit . . . . .	.151	—	.457	.333	.385	-.004	.131
10	Repeated Letters . .	.134	.118	—	.370	.343	.318	.043
11	Identical Pictures . .	.129	.136	.130	—	.177	-.040	.432
12	P.F.-S.F. Ratings . .	.151	.126	.134	.146	—	.464	.134
13	Alpha Frequency . .	.164	.162	.144	.164	.126	—	.046
14	Progressive Matrices.	.179	.176	.179	.146	.168	.189	—

multiple correlation of  $R = .519$  originally found for the four-test battery. These coefficients are not significantly different, which confirms that non-normality of bivariate distributions does not affect multiple correlation. The value of  $.606 \pm .091$  can therefore be accepted as a valid estimate of the relationship obtaining between the tests and the ratings.

We now turn to the results of factor analysis for some indication of the nature of the common element.

## RESULTS OF FACTOR ANALYSIS

Because not all subjects who were rated were able to complete the test battery, intercorrelations were obtained from samples of unequal size, the lowest  $n$  being 36, the highest 51. In view of the smallness of the experimental population, it was decided to use these rather than to calculate each  $r$  from the smaller sample which had done all the tests. The correlation matrix was factored by means of Thurstone's centroid method. After five factors had been extracted, only 13 of the residuals exceeded .1, the median value being .06. Factorisation was repeated five times, with successive estimates of communalities, at which stage the largest change in any of the communalities was only .06 (median .015). As the problem of the statistical significance of a factor has not yet been settled, and current significance tests can provide only approximate answers, particularly if  $n$  is small, we used as a rough guide the following formula suggested by Burt:

$$\chi^2 = \frac{1}{2} N [(\sum r_{is}^2)^2 - \sum r_{is}^4]; \quad d. f. = \frac{1}{2} [(n-s)^2 - (n+s)]$$

where  $N$  = number of subjects,  $n$  = number of tests,

and  $\sum r_{is}^2$  = sum of squares of  $s$ th factor loadings.

Factor extraction can be continued until  $P$  is larger than .50.

Application of this test was difficult because of unequal test samples, but even by taking  $N$  at its maximum of 50 cases, the second factor was found doubtful, and the third definitely not significant. All five factors

TABLE III  
Centroid Factor Matrix

No.	Tests	I	II	III	IV	V
1	Tapping I . . . . .	.34	-.38	-.31	-.20	.30
2	Tapping II . . . . .	.34	-.33	-.49	-.31	-.42
3	Crosses . . . . .	.77	-.16	-.21	.21	.15
4	Walking . . . . .	.21	.34	-.26	-.19	-.03
5	Talking . . . . .	.32	.56	.59	-.13	-.11
6	Writing . . . . .	.64	.05	-.11	.17	-.16
7	Adding . . . . .	.55	.33	-.15	-.09	-.20
8	Sorting . . . . .	.35	.12	-.23	.32	.11
9	Pursuit . . . . .	.60	-.08	.07	.24	-.06
10	Repeated Letters . . . . .	.56	-.20	.26	.50	-.18
11	Identical Pictures . . . . .	.63	.30	-.04	.07	.16
12	P.F.-S.F. Ratings . . . . .	.46	-.38	.27	-.04	.22
13	Alpha Frequency . . . . .	.37	-.63	.37	-.42	.11
14	Progressive Matrices . . . . . ?	.40	.43	.21	-.16	.16

were, however, retained to assist in rotating the first two axes to the most meaningful position permitted by the use of all data.

The centroid matrix is given in Table III.

Rotation was carried out according to Reyburn and Taylor's hypothetico-deductive method (18, 19) (See also Taylor, 22).

The first step in the application of this method is to formulate a hypothesis, on theoretical and experimental grounds, regarding the nature of a particular factor and its representation in various test variables. An axis is thereupon passed through the centroid of the variable or variables in which its saturation is expected to be highest. The direction cosines of this new axis, relative to the axes of the centroid matrix, are obtained by normalising the coordinates of this point.

If an orthogonal relationship between the factors is desired, the remaining axes must be rotated so as to preserve the direction cosines of the new axis and the orthogonality of the matrix. In the five-dimensional case, these conditions can be met by means of the following transformation, which satisfies the criteria of orthogonality:

$-p$	$q$	0	0	0
$qw$	$pw$	$-x$	$y$	$-z$
$qx$	$px$	$w$	$-z$	$-y$
$qy$	$py$	$-z$	$-w$	$x$
$qz$	$pz$	$y$	$x$	$w$

If the elements in the first column are put equal to the direction cosines of the new axis, numerical values for all terms can be found by solving the following equations:

$$q = \sqrt{1 - p^2}; w = qw/q; x = qx/q; y = qy/q; z = qz/q.$$

A similar procedure is followed for the remaining factors, the rank of the matrix being reduced by one in each case.

By means of this procedure, the entire communality of the test through which the first axis is passed is attributed to the factor represented by that axis. The solution is only acceptable (a) when the significant factor loadings of all the variables analysed, or measured on the new axis, are reasonable and intelligible in terms of the hypothesis which prompted the location of the axis; (b) when the meaningful interpretation of the other significant factors is not precluded by the location of any one of them. A certain amount of trial and error is therefore inevitable in this procedure.

The principal questions that factor analysis was required to answer were the following:

1. Does the relationship between the tests and the criterion include any aspects of speed or tempo of behaviour?
2. If it does, is there evidence that such speed elements are related to secondary function, the involvement of mental inertia being the decisive point?
3. Are there any aspects of speed of response that are not so related?

The presence of unrelated elements in the criterion would be particularly significant, for it would indicate that secondary function had been used by the assessors as a classificatory concept devoid of psychological unity, or that the hypothesis regarding secondary function which provided the starting point of our investigation required reformulation.

An orthogonal factor solution was attempted as this would most conclusively reveal whether our experimental data could possibly sustain a hypothesis of speed factors which form no part of the criterion, or of a criterion involving unrelated factors. After various abortive trials, the first axis was placed through Repeated Letters, the second through Tapping I. These tests were chosen because both correlate highly with the criterion, though they involve apparently distinct types of speed. Motor speed is the main element in Tapping I, but apart from the occasional ringing of repeated letters and of oculo-motor speed on the receptor side, it plays a negligible part in Repeated Letters. Performance in the latter is largely a matter of perceptual speed, which in turn appears to involve flexibility or freedom from mental inertia.

Table IV gives the transformation matrix required to retain orthogonality when rotating axis I to its new position. It also gives the final direction cosines for axis II. We did not proceed with rotation beyond this point as the remaining factors are not statistically significant and no hypothesis can be formulated regarding their location.

Before we try to interpret the factors obtained in this manner, reference

TABLE IV  
*Orthogonal Transformation Matrix  $T_1$  and Direction Cosines of Axis B in Final Position*

	A	B	C	D	E	B
I . . . . .	.668	.744	0	0	0	0
II . . . . .	-.238	.220	-.417	.801	.289	.607
III . . . . .	.310	-.287	-.320	.289	-.801	.632
IV . . . . .	.596	-.551	.289	.320	.417	-.478
V . . . . .	-.215	.199	.801	.417	-.320	-.059



must be made to another rotation according to the principle of simple structure, carried out independently by the second author without knowledge of the hypothetico-deductive solution. As psychological considerations need play no part in the use of this method, simple structure was sought for the entire centroid matrix, though again it was only intended to define the first two factors. An approximation to simple structure was achieved after six rotations, though orthogonality could not be maintained. Table V gives the transformation matrix and Fig. 1 shows

TABLE V  
*Transformation Matrix, for Rotation to Simple Structure*

	A	B	C	D	E
I . . . . .	.584	.316	.058	.185	.124
II . . . . .	-.164	-.706	.215	.270	-.207
III . . . . .	.078	-.008	-.425	.734	-.185
IV . . . . .	.791	-.522	-.849	-.587	-.348
V . . . . .	.008	.357	.221	.097	-.887

the location of factors A and B. The cosine of the angular separation between reference vectors A and B is -.110 and the correlation between the corresponding primary factors is -.29. This may account for the slight differences between the loadings obtained from the two reference frames

TABLE VI  
1. *Oblique factor matrix, obtained by rotation to simple structure;*  
2. *Orthogonal factor matrix (hypothetico-deductive)*

Tests	1. Oblique					2. Orthog.	
	A <sub>1</sub>	B <sub>1</sub>	C	D	E	A <sub>2</sub>	B <sub>2</sub>
1	.08	.59	.30	-.12	-.02	.04	.70
2	-.03	.36	.33	-.25	.68	.06	.47
3	.63	.30	-.05	-.16	-.04	.58	.53
4	-.10	-.09	.35	.05	.09	-.13	.08
5	.04	-.27	-.02	.71	-.04	.21	-.43
6	.41	.06	-.06	-.05	.08	.50	.23
7	.18	-.08	.19	.11	.24	.23	.08
8	.42	-.10	-.10	-.20	-.14	.30	.15
9	.57	.06	-.25	-.02	.14	.62	.13
10	.77	-.01	-.59	-.07	.06	.84	.00
11	.37	.01	.09	.14	-.14	.34	.14
12	.32	.51	-.09	.22	-.09	.41	.40
13	.02	.82	.10	.43	.16	.24	.52
14	.05	-.04	.19	.45	-.16	.10	-.02

determined by means of simple structure and hypothetico-deductive rotation. In further discussion,  $A_1$  and  $B_1$  will denote the factors obtained from rotation to simple structure,  $A_2$  and  $B_2$  the hypothetico-deductive factors.

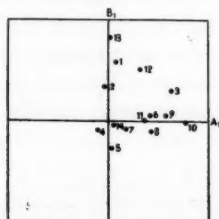


Fig. 1 Final plot of projections of tests on factors  $A_1$  and  $B_1$  (after 6 rotations).

Table VI gives both the oblique factor matrix and the two hypothetico-deductive factors. The principal loadings on the two pairs of factors with which we are concerned are compared in Table VII. It will be seen that the tests which have significant projections are virtually the same in each case and that differences are largely confined to the absolute and relative sizes of the loadings.

TABLE VII  
*Principal Loadings on Factors  $A_1$  and  $B_1$  (Simple Structure),  
 $A_2$  and  $B_2$  (Hypothetico-Deductive)*

Tests	$A_1$	$A_2$	Tests	$B_1$	$B_2$
Repeated Letters . . .	.77	.84	Alpha frequency . . .	.82	.52
Crosses . . . . .	.63	.58	Tapping I . . . . .	.59	.70
Pursuit . . . . .	.57	.62	P. F. Ratings . . . . .	.51	.40
Sorting . . . . .	.42	.30	Tapping II . . . . .	.36	.47
Writing . . . . .	.41	.50	Crosses . . . . .	.30	.53
Identical Pictures . .	.37	.34	Writing . . . . .		.23
P. F. Ratings . . . . .	.32	.41	Talking . . . . .	-.27	-.43
Alpha Frequency . . .	.02	.24			

Alpha Frequency has a small projection of .24 on factor  $A_2$  which may be significant, whereas on  $A_1$  its projection is virtually zero. Loadings which on  $B_1$  are of borderline significance, are sufficiently substantial on  $B_2$  to be taken into account in defining the factor. The agreement is so close that, despite the shortcomings of our experimental material, interpretation can be undertaken with some confidence. Before doing so we should mention that various other hypotheses were tried out, one of which merits some consideration. When the first axis was placed through P.F. Ratings, a factor resulted with the following significant projections:

P.F. Ratings .69; Alpha Frequency .80; Crosses .55; Repeated Letters .50; Tapping I .42; Pursuit .40; Writing .32 and Identical Pictures .28. Though this factor is superficially attractive in terms of our initial hypothesis about the nature of secondary function, it tells us little more than the correlation matrix and answers few of the questions to which the correlations gave rise. It could be interpreted as a temperamental speed factor related to level of excitability in the central nervous system, but in view of our ignorance about the validity of the criterion, the latter's projection on it provides no justification for identifying the factor as secondary function. Undoubtedly some of the tests in which it has saturations imply the occurrence of inertia effects; but the common element represented by the factor might equally well be receptor-effector speed, the connection of which with secondary function has still to be proved.

Factorially, the solution is unsatisfactory, for the following reasons:

(a) The second factor becomes difficult to interpret. With the axis through Tapping I, a bipolar factor results with positive loadings in Repeated Letters and Talking of .40 and .61, and negative loadings in Tapping I and Tapping II of -.50 and -.43 respectively. This might indicate automatic motor speed in opposition to the highly conscious, deliberate and adaptive motor response involved in talking; but the projection of Repeated Letters on such a factor is difficult to account for, unless the factor is interpreted as automatic speed versus deliberation and judgment, in which case Identical Pictures, Progressive Matrices, Pursuit and Sorting should also have been represented. It might of course be held that no interpretation of the second factor is called for, as it is of very doubtful statistical significance; but the clear-cut interpretation which can in fact be given to factor B argues against this.

(b) It is not in agreement with the simple structure solution. By itself this does not render a hypothetico-deductive solution invalid; but as one can be found which does agree, and which is capable of more consistent interpretation, that one should be preferred, even if it lacks the immediate psychological attraction of the former.

(c) As the factorial data can sustain an interpretation which splits the criterion into two independent parts, one cannot validly prefer another interpretation which retains its unity. Either one must conclude that the experiment has neither proved nor rejected the hypothesis which it set out to test; or one must accept the solution which is factorially more cogent, provided it makes psychological sense. In the discussion which follows, the latter possibility will be examined.

## DISCUSSION

Although factor A represents all the tests in which speed of performance is restricted by visual requirements, it should nevertheless not be defined as a speed of visual perception factor. Its saturation in Primary Function Ratings, which were obviously not based on the visual responses of the subjects rated, argues against this interpretation. So does the fact that Crosses, in which the visual element is minimal, has loadings of .58 and .63 in the two analyses, whereas Identical Pictures, in which visual discrimination predominates, has loadings of .37 and .34 only. Thurstone repeatedly refers to the temperamental implications of his visual factors. Thus of the speed of perception factor, which in the first two primary mental abilities studies had its highest saturation in Identical Pictures, he states: "It is ... quite likely that there are temperamental associates with this factor" (25, p. 1). Of the speed and flexibility of closure factor, defined in his later studies he says: "it would be of special psychological interest to determine whether these factors can be found to be descriptive of intellectual or temperamental characteristics of the individual that transcend the purely perceptual effects" (23, p. 106). This possibility was examined by Pemberton (16) who found that all three factors were associated with distinctive temperamental behaviour patterns.

Perceptual speed is described by Thurstone as "the ability to compare visual configurations ... the ability that is involved in scanning a page, for example, to find a particular number or name" (25, p. 1). Speed of perceptual closure depends both on the ability to organize a visual field into a single percept and on the ability to hold this percept in mind without losing its imagined identity or shape. Flexibility of perceptual closure involves "the ability to shake off one set in order to take a new one ... Freedom from *Gestaltbindung* might be an appropriate description in that it implies flexibility in manipulating several more or less irrelevant or conflicting gestalts" (23, p. 111). Identical Pictures, described by Adkins and Lyster (1) as a factorially complex test, involves all three of these factors. They found it had loadings of .31 on perceptual speed, .48 on speed of perceptual closure and .31 on flexibility of closure. In a study of group tests of mechanical aptitude, Thurstone (24) also obtained the highest loading for Identical Pictures on speed of closure. No information from other studies is available regarding the factor content of Repeated Letters, but introspective analysis suggests that all three factors described above are involved to an even greater extent in this test than in Identical Pictures. The task is one in which printed material is scanned to find

particular items. This concerns the perceptual speed factor. Both the background material and the items to be marked are relatively unstructured. To work fast, one should look out for pairs or triplets, rather than examine each letter with reference to its neighbour. A mental set must therefore be established to perceive particular visual configurations which, though different, possess the common element of repetition. This concerns speed and strength of perceptual closure. The "repetition gestalt" has no fixed visual form but varies according to the constituent letters. It has also to be maintained against the competing gestalten of *www*, *ppp*, etc., which have a symmetry and order of their own. Flexibility of perceptual closure is therefore also involved.

It is suggested that factor A, which accounts for a very large proportion of the communality of Repeated Letters (59% in the simple structure and 71% in the other solution) is a common temperamental element, associated with these three perceptual factors and that it represents flexibility of behaviour, determined ultimately by inertia which is the essence of the concept of secondary function. The connection between freedom from inertia and ease and flexibility of closure seems plausible in the light of introspective analysis of the tests associated with these factors. The ability to interrupt and restart a visual scan, which is so important a determinant of performance in the Repeated Letters test, also fits this interpretation. It is worth recalling Stephenson's suggestion (21) that, paradoxically, low *p* scores in tests of the alternation type (rapid switching from an X to a Y activity, as in the  $S_2$  test) could result from the inertia effects of a mental set to perceive successive alternate forms as a new pattern. The paradox disappears if speed of perceptual closure, whereby the alternate forms are turned into a new gestalt, is considered to be the operative element.

The relationship between freedom from inertia and perceptual speed is less obvious. Dark Adaptation, found by Wiersma (27) to be correlated with secondary function has, however, a high weight in the composite for perceptual speed (weighted score, based on the tests with highest projections on the primary factor) (Thurstone, 23, p. 113). Pemberton (16) did not find a very decisive association between perceptual speed and temperament, but she formed the impression that those who scored high on this factor were unsystematic and reacted immediately to outside stimuli. "The high *P* group state that they often act on the spur of the moment; like variety and contrast; often act without considering the consequences; do not act slowly and deliberately; and their moods change without apparent reason." This description fits in well with the primary

temperament and therefore supports the hypothesis that the temperamental component in perceptual speed is related to freedom from inertia.

Finally there is Thurstone's observation that the composites for perceptual speed, speed of perceptual closure, flexibility of closure and the P.M.A. space factor are positively correlated, the median  $r$  being between .51 and .40 (23, pp. 121—123). Thurstone thought that the perceptual primaries were essentially uncorrelated, and that this departure from expectation was due to contamination by extraneous factors in the composites. It may well be, however, particularly in view of his later references to temperamental components in these tests, that a temperamental factor of the kind suggested by our experimental data, was partly responsible for the high correlations.

The remainder of the loadings on factor A present a consistent picture. Its high saturation in Crosses is due to the fact that in order to achieve speed, the subject must convert a sequence of brief starts and stops and alternate direction reversals into a movement rhythm. This would seem to depend on the establishment of psychomotor closure and to be favoured by freedom from inertia. In the Pursuit test flexibility is evident, not only in the tracing of the lines, which is a comparatively minor aspect of the test, but principally in the segregation of each line as a separate entity from its confusing background. Both the perceptual closure factors appear to enter into this task. The significant projections of Writing and Sorting are less easily accounted for. Both contain much specific material, the latter being an associative learning task, the former depending on established motor habit. One assumes that when perceptual restrictions are imposed on the free flow of motor responses, the degree to which this flow will be retarded will depend on freedom from inertia.

Alpha Frequency was expected to have a substantial projection on factor A, for it is positively correlated with Repeated Letters and has the highest correlation with Primary Function Ratings. Yet we find a zero projection on factor  $A_1$ , a projection of only .24 on factor  $A_2$ . Even if we accept the latter as significant, in view of other experimental evidence presented by Mundy-Castle which links alpha frequency with central neural inertia phenomena, the amount of variance accounted for on this basis is too small for the confirmation of this linkage to be convincing.<sup>2</sup> The smallness of the loadings of the criterion on both  $A_1$  and  $A_2$  (.32 and .41) also requires some explanation. A probable contributory cause is

<sup>2</sup> An investigation has just been completed at the N.I.P.R. by Reuning in which a correlation of .545 (sign. at 1%) was found between alpha frequency and flicker threshold previously shown to be related to secondary function.

the larger error variance to be expected in the case of ratings because measurement is indirect and is consequently liable to errors of observation and interpretation which do not occur in objective test performances. More important is the spreading of its communality over both factors A and B.

The nature of factor B is most clearly defined by a relatively unstructured action like tapping at maximum speed. Performance in Tapping I, which has high loadings on both  $B_1$  and  $B_2$  (.59 and .70) involves a minimum of perceptual content and no adaptive purpose. In marked contrast with Tapping, performance in Talking is essentially conscious, always adaptive and dependent on the participation of association areas. Its negative loadings on factors  $B_1$  and  $B_2$  (-.27 and -.43) therefore suggests that factor B represents unstructured motor speed, inimical to the expression of considered and articulated thought. Even in casual conversation, apparently, it is not possible "to shift one's mind into neutral and let one's tongue idle on".

Further confirmation is provided by the considerable projection of Alpha Frequency on factor B (.82 and .52). Alpha frequency has been shown to be related to involuntary tremor movements (Jasper and Andrews, 14). It is considered to be determined, at least in part, by latency in central circuits. Unstructured motor speed, as in tapping as rapidly as possible, is limited by the duration of the refractory phase in the particular central neurones concerned in this reaction, and probably by no other restrictions. The motor speed represented by factor B would therefore appear to derive from a general latency phenomenon related to level of excitability in the central nervous system. That it has temperamental implications is shown by its fairly substantial saturation in Ratings (.40 and .51).

As factors A and B are independent, it must be concluded that the definition of secondary function which formed the starting point of this investigation was too wide and that some aspects of motor speed should have been excluded from its behavioural implications. On the other hand, some features of the postulated behaviour pattern are confirmed. The definition of factor B excludes the possibility that the correlations represented by factor A resulted from effector speed as a common element, and as the possibility that they represent visual receptor speed has already been rejected, it follows that flexibility and mobility of behaviour are truly part of the primary-secondary function behaviour pattern through the common central element of freedom from inertia.

Unstructured motor speed must, however, be treated as a separate



dimension of temperament. One ventures to guess that outside the test situation it shows itself behaviourally in the form of "quickness" of movements and gestures. That Walking has no significant projection on it ( $-.09$  and  $.08$ ) could have resulted from the error introduced by not taking leg length into account when determining walking speed; or it could indicate that in walking, as in other expressive movements, constitution tends to be obscured by habit.

Supporting experimental evidence for the proposed definition of factor B is difficult to find because of differences in experimental design and statistical methods used by the many investigators of speed and psychic tempo. Cattell (8) in a comprehensive survey of this work, describes two "source traits" of temperament which together cover approximately the same domain as factor B. These are (a) "fluency", which he defines as "speed of spontaneous productivity or reproduction through association, granted that no demand is made upon discriminatory power—i.e. that individual differences are not due to differences in judgment"; (b) tempo, which is defined as "natural speed and rhythm ... when no urgent incentive is operating". He considers these two to be correlated, and to represent such test performances, amongst many others, as tapping at preferred speed, writing, cranking, card sorting and fluency of verbal association, independent of verbal comprehension and considered speech. For repetitive motor responses in which speed is deliberately aimed at, and amongst which tapping at maximum speed is included, he postulates a separate speed factor. He shows, however, (a) that maximal and voluntary tempos are correlated; (b) that in other ways speed, fluency and tempo factors overlap; (c) that they are independent of motor perseveration of the hindrance type. The resulting picture, as presented by Cattell, is far from clear; but it lends some support to the hypothesis of an unstructured motor speed factor in which secondary function plays no part.

Though psychologically factor A (flexibility) and factor B (unstructured motor speed) are sufficiently distinct, their definition has not greatly clarified our knowledge of their physiological foundations. Little is known about the relationships obtaining between the constituents of level of excitability, of which both factors appear to be manifestations, and discussions in terms of "after discharge" and "latency" have no meaning, without precise definition of these concepts. A possible distinction between inertia, and energy endowment, the former related to factor A, the latter to factor B, might provide a useful hypothesis for further work. Before this is undertaken, however, the two factors, thus tentatively defined, require



confirmation in a larger population, and by means of an experimental design which will take account of the many questions left unanswered in the present investigation.

Meanwhile the prediction of secondary function by means of psychomotor, perceptual and other objective tests will have to be approached with care. The high multiple correlation obtained between a number of these tests and the primary-secondary function ratings is contaminated by the intrusion of the motor speed factor. Here, too, further studies are necessary, with more tests likely to involve factor A and with a more precise criterion.

Finally, speed of work, hitherto used as one of the criteria in the observational assessment of secondary function, will have to be more carefully circumscribed, to prevent the disturbance of measurements by another variable which, though important temperamentally, is likely to play a much more narrow and specific part in the growth and function of the personality.

#### SUMMARY AND CONCLUSIONS

1. A battery of psychomotor and perceptual tests correlates with a paired comparisons rating, serving as a criterion of secondary function, to the extent of  $R = .606 \pm .091$ .

2. Factor analysis shows that this criterion has projections on two independent factors, of which only one, tentatively defined as "flexibility", can be related to mental inertia and thus to secondary function. The other, representing unstructured motor speed, merits further investigation as an additional dimension of temperament.

3. It is probable, therefore, that the behaviour pattern to which primary-secondary function gives rise excludes such elementary aspects of speed of response as quickness of movements and gestures, but that it includes speed resulting from mobility and plasticity of behaviour at a more discriminative level.

4. The extent to which secondary function can be measured by means of objective tests needs to be studied anew, as the multiple correlation obtained in this investigation refers to a complex criterion, involving other factors besides secondary function.

5. Secondary function is shown to have wider behavioural implications than perseveration, defined in the older sense of mental inertia, and to differ conceptually from the more recent definition of perseveration as "disposition rigidity".

6. Both unstructured motor speed and flexibility have saturations in alpha frequency, indicating their relation to level of excitability of the central nervous system. The physiological differentiation of the two factors remains obscure.

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## THE ELECTROENCEPHALOGRAM IN RELATION TO TEMPERAMENT

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The Heymans-Wiersma theory of temperament involves three basic variables: Secondary Function, Emotionality and Activity. Detailed accounts of this theory, together with its history, are available in works by Heymans (20), Wiersma (54), Biesheuvel (5, 7) and Baehr (2). The present paper is concerned with the concept of Secondary Function in relation to the electroencephalograms (EEG's) of normal adults.

Every conscious event may be described as possessing both a primary and secondary function. Its primary function is its immediate experience, its secondary function the after-effect it is capable of exerting after receding from consciousness. Thus, through its secondary function, a past mental event can spontaneously or associatively reinstate itself, can modify the character of some new primary content, or exercise a "braking" effect on such primary content (Biesheuvel, 7). The latter effect has been called "perseveration" (*p*) in psychological literature and dealt with as a separate phenomenon. Biesheuvel stresses that *p* is only one of the manifestations of secondary function and that there are others, not always included in the definition of *p*, which are equally important. The general effect of secondary function is to give continuity and integration to mental activity, since it favours persistence of attention and rate of work, relative stability of moods and interests, and action in the light of past experience. Biesheuvel has likened it to a "neural governor" (7, p. 13).

The degree of secondary function present in individuals varies over a continuous scale. At one extreme, where it is relatively lacking (it can never be completely lacking), the personality is characterised by an extensive but shallow conscious field, dominated by primary experience. At the other extreme, where primary conscious content is dominated largely by secondary function, the conscious field is given greater depth

due to the resultant reduced stimulability and more frequent evocation of past experience.

In the present study, temperament is regarded as a heredito-constitutional basis for personality development, and Heymans (20) has produced considerable evidence in support of the genetic origin of secondary function. Temperament variables cannot be measured directly in the sphere of personality, since their existence and operation can only be inferred from personality traits or behaviour which are products of the interaction between constitution and the environment. The assessment of temperament is nevertheless of considerable importance, both in order to gain insight into the origins and interrelations of personality attributes, and for the prediction of future behaviour.

Assessments can be made by either a subjective or objective method. The former involves rating by a trained tester and is open to numerous difficulties, although such a scheme has been utilised and reported on by Biesheuvel (7). The test-situation is made as little subjective as possible by using partially structured performance tests which serve as a setting for the observation of overt behavioural qualities. These observations are recorded on a rating scale, by means of which the strength of a number of temperament variables can be computed. The main difficulties lie in the selection and training of testers, and in the inescapable occurrence of testers' bias and halo effect. The alternative approach of objective measurement is clearly a more satisfactory one, provided that some valid and reliable measure can be found. The secondary function variable has been studied in several ways by this means. Wiersma (54) and Biesheuvel (6) found that individual differences in critical flicker fusion frequency (CFF) reflected a relationship between degree of sensory perseveration and behavioural strength of secondary function, although this has been disputed by Jones (26). Wiersma (54) also studied secondary function by the method of free association, where all the images occurring as a result of a stimulus-word were recorded, so that the strength of after effect of the stimulus could be observed. In addition, both Heymans (20) and Wiersma (54) showed that the checking action of an initial strong light stimulus on the threshold of a subsequent weaker one varies according to strength of secondary function. Further studies involving other senses in a similar manner were also undertaken by Wiersma (54), who refers to them as "inhibition-experiments" (p. 54). The findings in these early investigations were not treated in a comprehensive statistical fashion, but the consistent results strongly suggest the possibility of measuring secondary function by objective psychophysiological methods.

Since there were several indications that the frequency of alpha rhythm in the adult EEG might be related to degree of secondary function, it was decided to test this hypothesis experimentally. Foremost amongst these indications was the report of Davis (13) that depressed manic-depressives (MDD's) tend to show frequencies of alpha rhythm in the EEG at 10 c/sec. or slower, whilst manic manic-depressives (MDM's) tend to show alpha frequencies a 10 c/sec. or faster. Since primary functioning behaviour is most evident among MDM's and secondary functioning behaviour among MDD's (Heymans, Wiersma, *loc. cit.*), it was considered reasonable to expect similar differences in a normal population, namely that low alpha frequencies would be associated with secondary functioning temperaments and high alpha frequencies with primary functioning temperaments. Indirect support for this view is implicit in the theory advanced by Pitts and McCulloch (43) and Walter (Hill and Parr, 21, Ch. VII) that one of the functions of the alpha rhythm may be that of scansion of the visual cortex for information, since such a view implies that rates of visual perception vary according to scanning frequency, whilst cerebral after-effects of visual stimuli are shorter with higher alpha frequencies. These implications tie up well with the reports relating to CFF mentioned above.

#### METHOD

In view of the difficulties of accurate assessment of temperament in unknown subjects, it was decided to utilise members of the staff of the National Institute for Personnel Research both as subjects and assessors. Full details of the method of temperament rating are given by Biesheuvel and Pitt elsewhere in this issue. EEG's of 40 staff members were recorded by an Ediswan six-channel electroencephalograph. Their mean age was 24.10 years, standard deviation 4.67 years. Silver silver-chloride saline pad electrodes were secured to all main areas of the scalp (frontal, parietal, temporal and occipital) by an elastic and whalebone cap, and all recordings were bipolar, i.e. measurement was taken of potential differences occurring between electrodes on the scalp. Subjects were tested in a moderately lit room whilst relaxing full-length on a couch, no recording lasting less than 20 minutes. The alpha rhythm was defined as "activity at any frequency between 8 and 13 c/sec. located to parieto-occipital areas and normally being attenuated by opening the eyes". Alpha frequency was measured with a transparent ruler, 1.5 or 3 cms. corresponding to 1 sec. according to paper speed setting, both settings being employed in all these recordings. Walter (Hill and Parr, 21, Ch. VII) has pointed out that the alpha rhythm usually possesses two or more separate components, and our own observa-

tions have been similar, both by means of direct measurement and with the aid of automatic frequency analysis (cf. Baldock and Walter, 3). Cohn (10) has made similar observations. It was thus necessary to utilise mean alpha frequencies for the purposes of this experiment, so that a subject showing alpha frequencies at 8, 9 and 10 c/sec. was recorded as possessing a mean alpha frequency of 9 c/sec. In all subjects tested alpha frequencies were within  $\frac{1}{2}$  or 1 c/sec. of one another, in other words no subject possessed rhythms at, say, 8 and 11 c/sec. without intermediate frequencies also present. There was thus no difficulty in arriving at a mean alpha frequency for each subject.

Frequency of alpha rhythm under resting conditions is very constant, and in none of the subjects tested did the mean show any observable variation during relaxed periods of recording time. This accords with the early reports concerning consistency of alpha frequency, where variability was found from day to day, month to month and even longer intervals, seldom to exceed 2 c/sec., usually less than 1 c/sec. (Travis and Gottlob, 48, 49; Lindsley and Rubinstein, 32; Lindsley, in McV. Hunt, 35). It is probable too that to these workers variability appeared greater than it was, since at this time the multirhythmicity of alpha activity was not appreciated. Thus an apparent shift in frequency from 9 to 10 c/sec. may in fact be due to variations in dominance or amplitude of two separate alpha components. In summary, Walter has stated that in constant conditions the various alpha components "vary very little in frequency and even in states of stress such as excitement or hyperpnoea-alkalosis their frequency does not change by more than 0.5 to 1 c/sec." (Hill and Parr, 21, Ch. VII, p. 220).

Since a parallel investigation involved objective measures designed to assess psychic tempo, scores from these tests were also studied in relation to alpha frequency. The nature and details of scoring these other tests, together with a factorial study of all these various measures, is presented by Biesheuvel and Pitt (*loc. cit.*). The actual speed tests were as follows:

Tapping fast,

Tapping at own preferred speed,

Walking to and from an observation point,

Writing a specific passage,

Adding simple figures,

Talking (casual conversation),

Drawing crosses on squared paper, mean of fast and preferred speed,

Thurstone's repeated letters test,

Thurstone's pursuit test,

Thurstone's identical pictures test,  
 Sorting numbered discs into appropriate compartments,  
 Fitting of patterned blocks into a formboard.  
 None of the subjects was aware of the purpose of the tests.

## RESULTS

Small sample product moment correlations were calculated between mean alpha frequencies, ratings and speed test scores. Five were significant as shown in Table I, the remainder not being significant. Individual mean

TABLE I

*Product moment correlations between alpha frequency, temperament ratings and speed tests*

	r	N	P	Whether Significant
Function Ratings. . . . .	.464	40	<.01	Yes
Tapping fast. . . . .	.403	32	<.05	Yes
Tapping own speed. . . . .	.381	32	<.05	Yes
Walking. . . . .	-.332	38	<.05	Yes
Writing. . . . .	.146	38	>.05	No
Adding. . . . .	-.099	36	>.05	No
Talking. . . . .	.091	35	>.05	No
Crosses. . . . .	.149	39	>.05	No
Repeated Letters. . . . .	.318	40	<.05	Yes
Pursuit. . . . .	-.004	39	>.05	No
Identical Pictures. . . . .	-.040	37	>.05	No
Sorting. . . . .	-.032	38	>.05	No
Formboard. . . . .	.058	39	>.05	No

alpha frequencies and corresponding temperament rating scores are shown in Table II.

Distribution of mean alpha frequencies is normal at the 1% level of rejection ( $\sqrt{b_1} = 0.849$ ,  $\alpha = 0.754$ , cf. Geary and Pearson, 15). Tests of normality of all other distributions are shown by Biesheuvel and Pitt. Since two of these, Walking and Repeated Letters, are not normally distributed and show significant product moment correlations with alpha frequency, Kendall's (27) rank correlation was also calculated. The values are as follows:

Alpha frequency and Walking:  $\tau = .193$ ,  $\frac{s-1}{os} = 1.598$ ,  $P > .05$

Alpha frequency and Repeated Letters:  $\tau = .268$ ,  $\frac{s-1}{os} = 2.306$ ,  $P < .05$



TABLE II  
*Individual mean alpha frequencies and temperament rating scores*

Temp. Rating Score.	Alpha Freq. (c/s.)	Temp. Rating Score	Alpha Freq. (c/s.)
.83	10.25	25.49	11.0
7.15	10.5	26.24	9.5
7.76	9.0	27.77	10.5
8.53	9.5	28.68	10.0
8.84	9.5	29.60	12.0
9.28	10.5	29.72	12.5
10.09	10.0	30.51	9.5
11.21	10.0	30.72	10.5
12.42	8.5	31.68	10.5
14.23	10.5	34.37	13.0
15.87	9.0	35.24	10.5
16.41	10.5	35.73	10.0
18.27	10.5	35.93	9.5
18.37	9.5	36.38	11.5
18.59	10.5	37.98	11.5
18.88	10.0	38.93	10.0
20.68	10.0	40.86	11.5
21.23	10.0	43.28	11.0
21.37	11.25	44.12	10.75
24.31	11.0	46.20	11.0

The product moment correlation between alpha frequency and Walking is therefore probably misleading, whilst that with Repeated Letters is confirmed.

#### DISCUSSION

There is considerable evidence to suggest that the chief characteristics of the EEG are strongly influenced by genetic factors (Davis, 12; Loomis, Harvey and Hobart, 33; Lennox, Gibbs and Gibbs, 31; Gottlieb, Ashby and Knott, 18; Lennox, 30; Mundy-Castle, McKiever and Prinsloo, 42), and the finding that alpha frequency is significantly correlated with ratings of primary-secondary function suggests that these were derived in part at least from elements of behaviour determined by heredity. It therefore seems legitimate to infer that they were ratings of temperament as defined in the introduction to this paper. Biesheuvel and Pitt (*loc. cit.*) have shown by factorial analysis that two factors are apparently involved in these ratings, factor A corresponding to "flexibility" or "freedom from inertia", and factor B corresponding to "unstructured, non-discriminatory motor



speed". The former could be a manifestation of secondary function. Both are regarded as temperament variables, with alpha frequency showing loadings on each, high on B, but of borderline significance on A. In considering the relationship of the alpha rhythm to temperament, in particular to these two factors, it is necessary to examine the probable functional role of the alpha rhythm in the determination of behaviour.

The possibility that the alpha rhythm may possess a scanning function has already been mentioned. According to this theory, the blocking response of the alpha rhythm during visual stimulation is due to activation of the brain cells scanned by the alpha rhythm, with the result that scanning waves are reflected back with shorter delays proportional to the shape and complexity of the visual stimulus, thereby transforming a spatial complex into a temporal pattern. This means that lack of activation of the visual areas invokes maximal amplitude of alpha rhythms, and helps to resolve the apparent paradox of maximal activity associated with resting states (Walter and Walter, 52). Evidence in favour of such a scanning process has been produced by Walter and Walter (53) and Walter (Richter, 45, Ch. V) in their studies of the central effects of rhythmic photic stimulation, whilst Mundy-Castle (37) has recently demonstrated that the incidence of beta activity (14—30 c/sec.) tends to be inversely proportional to the amount and amplitude of alpha activity, this being predicted on the scanning hypothesis. In addition, McCulloch (34) has shown that the theory is compatible with known physiology and anatomy of the visual areas of the brain, and that a similar mechanism may operate in auditory perception. Also relevant are the observations of Goldman *et al.* (16) concerning travelling waves in the brain, seen in slow motion-pictures of toposcopic cortical displays. They concluded that the observed pictures bore out the suggestion that the alpha rhythm represents a scanning operation in the brain, although they added that there was no certainty about this.

Walter (51) has pointed out that such a scanning system would require a special mechanism to regulate and synchronise efferent responses with the time base on which afferent signals are coded. It would therefore be expected that rates of limb movement will be in some way related to alpha frequency, and that there will be some central timing device which can control effector activity in accordance with habitual rates of reception in the nervous system. In fact, both Bates (4) and Kibbler and Richter (28) have demonstrated by different methods that the release of centrifugal signals and the moment of initiation of voluntary movements bear a close relation to the phase of the alpha rhythm. In addition, Jasper and

Andrews (24) have shown that there is a close correspondence between spontaneous tremor movements and the alpha rhythm, the authors concluding that normal control of spontaneous movement is probably due to a delicate integration of cortical and subcortical discharging centres, the extent of correspondence being dependent on the degree to which cortical rather than subcortical centres are in charge. These considerations are supported by the significant correlations between alpha frequency and fast and preferred tapping speeds, and offer a satisfactory explanation for the high loading of alpha frequency on Biesheuvel and Pitt's factor B, attributed to unstructured motor speed with a minimum of perceptual content and no adaptive purpose.

To turn to the problem of factor A or flexibility, separate from the pure speed of factor B and more closely akin to the concept of Secondary Function, although the loading of alpha frequency on factor A is low, there are several reasons for regarding it as meaningful. These are derived chiefly from ancillary investigations from our own laboratory and all point in a common direction suggesting associations between alpha frequency, secondary function and what might be termed the general level of excitability within the central nervous system. The relevant data are as follows:

(1) Since the observation of Davis (13) concerning alpha frequency in manic-depressive psychosis was one of the starting points of the present investigation (see Introduction), it was necessary to be quite certain as to its validity. EEG examinations were therefore made of 117 manic-depressive patients. Significant associations were found between cases with a history of predominantly manic phase and relatively high alpha frequency, and cases with a history of predominantly depressed phase and low alpha frequency (Hurst, Mundy-Castle and Beerstecher, 23). This finding confirmed that of Davis, and it is suggested that the relationship is attributable to the temperamental differences in such patients, that subjects who are primary functioning and develop manic-depressive psychosis are more likely to be manic than depressed, whilst subjects who are secondary functioning are more likely to be depressed than manic, these differences being reflected in frequency of alpha rhythm.

(2) An investigation was undertaken to test the hypothesis that individual differences in adaptation of the galvanic skin response (GSR) to repetitive auditory stimulation may be related to a temperamental factor underlying differences in excitatory/inhibitory balance (Mundy-Castle and McKiever, 41). The study revealed a significant correlation between GSR adaptation rate and alpha frequency in subjects with "Stable" GSR records

i.e. records showing no spontaneous or endogenous resistance changes: low alpha frequencies are accompanied by rapid adaptation, high alpha frequencies by slow adaptation. It is postulated that these differences in adaptation rate are related to degree of secondary function, and that the findings are comparable to those of Heymans and Wiersma in their inhibition-experiments (see Introduction).

(3) Significant associations have been established between alpha frequency and certain characters of the EEG response to rhythmic photic stimulation (flicker) (Mundy-Castle, 38, 39). Normal adults with high alpha frequencies are more likely to display high frequency flicker following (i.e. responses at 14–26 c/sec. to flicker at 14–26 f/sec.) and evoked 2nd and 3rd harmonics (i.e. responses at double and treble the stimulus frequency) in their EEG response pattern, than are subjects with low alpha frequencies. Since similar observations were found in the flicker response characteristics of manic-depressive patients (Hurst, Mundy-Castle and Beerstecher, *loc. cit.*) and of subjects with stable GSR records (Mundy-Castle, 40), in each case in accordance with expectation, the general conclusion is that these differences are most likely to be related to degree of secondary function. In addition, the differences must be related to overall recovery times within visual circuits and as such to individual overall excitability characteristics within the central nervous system, thereby giving some clue to the probable physiological basis of secondary function.

(4) Confirmation of the above indications has been independently provided by Gastaut *et al.* (14), who report that "calm" individuals have a slow (8–10 c/sec.), high voltage alpha rhythm, "nervous" individuals a high frequency (10–13 c/sec.), low voltage alpha rhythm. They demonstrated that a fast alpha group possessed higher levels of cortical excitability than a slow alpha group, and claim a relationship between alpha frequency and temperament. Whilst the exact nature of their assessments of the latter is not detailed, it is clear that they did not rely on speed alone. The following terms were used to describe their subjects with high alpha frequencies: "*nerveux*", "*actifs*", "*agités*", "*excités*" and "*hypomaniaques*", whilst for low alpha frequencies the terms "*d'amortissement*", "*tranquilles*", "*calmes*", "*debonnaires*" and "*lymphatiques*" were used. The resemblance between these terms and those connoted by secondary function is striking.

(5) In a preliminary investigation, Reuning (1955), in association with the author, has found a significant correlation between alpha frequency and CFF as measured by a new apparatus. The value for  $r$

was .545, this being significant at the 1% level ( $N = 23$ ). Low alpha frequencies were associated with low CFF's, high alpha frequencies with high CFF's. Reports of relationships between CFF and secondary function are discussed in the Introduction, and this finding adds further weight to the postulated association between alpha frequency and secondary function. The finding also confirms a prediction based on the theory that the alpha rhythm acts as a scanning generator, namely that the number of separate stimuli perceptible within a second will vary directly with alpha frequency.

(6) Lastly, it should be noted that alpha frequency is significantly correlated with Repeated Letters, this showing the highest loading on factor A and not appearing in factor B. Biesheuvel and Pitt maintain that Repeated Letters is a measure of freedom from inertia, this being closely associated with secondary function.

The general conclusion derived from these considerations is that there is a logically coherent body of evidence to support the hypothesis that alpha frequency is related to degree of secondary function, and that secondary function is related to the overall excitability characteristics of at least the central nervous system, and possibly the nervous system as a whole. A more detailed discussion of this problem has been presented elsewhere (Mundy-Castle, 40).

If the demonstration of two independent factors both related to alpha frequency and temperament is valid, it would be expected that at least two independent physiological variables are operative in the expression of alpha rhythm. Alpha frequency itself is in fact probably determined by a number of processes, many of which can be subsumed under the general heading of central excitability characteristics. Behind these lie differences in biochemical constitution, a subject beyond the scope of this paper. It should be noted, however, that alpha frequency varies with a large number of biochemical and pharmacological stresses (Heppenstall and Greville, Chs. V and VI, in Hill and Parr, 21), whilst Hoagland (22) has shown that it may be related to rate-limiting chemical pacemakers in the form of specific enzyme systems controlling cellular respiration.

In an attempt to identify the location and nature of the physiological variables which may account for the relationship of alpha frequency to the two independent factors, it is helpful to consider the different kinds of behaviour characteristic of factors A and B. The former, flexibility, enters into behaviour involving discrimination, integration and control. The behavioural aspects of factor B, or unstructured motor speed, emerge when high level integrative operations are abandoned and movements are

directed by spontaneous or non-discriminative processes. In other words, the first factor involves high level cortical integration centres and diminishes the behavioural manifestations of the basic speed and rhythm of central nervous processes reflected by factor B, although the rate of complex discriminating operations will still be governed by cortical excitability characteristics and as such related to alpha frequency. One of the features of high level discriminative behaviour is that of attention or mental set, and the relationship between this and alpha activity has long been recognised (e.g. Knott, 29, Williams, 55, 56). Not only may the alpha rhythm fail to block if attention is not focussed upon a visual stimulus (Adrian, 1), but attention to any sensory stimulation may be accompanied by alpha blocking (without awareness of visual images), so that alpha mechanisms may very probably involve non-visual operations, provided these require attention or readiness for discrimination (cf. Knott, 29; Harrison, 19; Darrow, 11). It should also be noted that Jasper (25) has suggested that the diffuse thalamic reticular system may regulate alpha function and is a possible central integrative mechanism for direction of attention.

These considerations enable the following possible explanation of the present findings: behaviour comprising manifestations of factor A will predominate when attention and readiness for discrimination is maximal, in which case alpha rhythm will be minimal; conversely, behaviour characterised by factor B will predominate when attention is minimal and alpha rhythm will be maximal. Naturally neither factor will be a sole determinant of behaviour. This hypothesis is open to experiment, although the numerous factors contributing to the wide range of possible alpha patterns (cf. Golla, Hutton and Walter, 17; Moses, 36; Darrow, 11; Saul, Davis and Davis, 46; Short, 47) will inevitably complicate the results.

Such a hypothesis implies two independent physiological variables, one cortical, the other probably thalamic. This accords with the conclusions of Bishop (9) concerning a reciprocal relationship between the activities in thalamus and cortex, in which each may be capable of driving the other, although in resting conditions the thalamus probably drives the cortex. That there is some degree of independence or "semi-independence" between the two has been discussed by Walker (50) and several others in a symposium on thalamo-cortical relationships, with the significant observation that such semi-independence allows the thalamus to react to extraneous stimuli without affecting cortical activity. The thalamus therefore appears to be a likely origin for a great part of the electro-physiological background to factor B, particularly since it probably drives

cortical rhythms in resting states. It also seems true to state that both factors are manifestations of a common underlying neurophysiological substrate related to overall level of excitability in the central nervous system.

#### SUMMARY

This paper describes an experiment in which a significant correlation was found between ratings of the temperament variable Secondary Function and frequency of alpha rhythm in the EEG. Subjects rated as secondary functioning possess lower alpha frequencies than subjects rated as primary functioning.

The experiment included tests of speed and psychic tempo; some of the scores from these are also correlated with alpha frequency. A factorial analysis revealed two factors underlying these results, one (factor A) apparently closely related to Secondary Function, the other (factor B) to unstructured motor speed. Alpha frequency shows loadings on both, low on the first, high on the second. It is suggested that these factors are manifestations of temperament.

The possibility that the alpha rhythm may behave as a scanning generator is discussed, and evidence is presented which suggests that there may be a central timing device, manifested in the alpha rhythm, which assists in the coordination of efferent with afferent signals. These considerations are held to offer a satisfactory explanation for the high loading of alpha frequency on factor B.

Evidence from other studies is presented to substantiate the apparent relationship between alpha frequency and Secondary Function (identified with factor A) and to suggest that this temperament variable is related to an overall central nervous excitability characteristic.

An attempt is made to account for the apparent independence of factors A and B by considering the relationship between alpha rhythm, attention and mental set. It is suggested that behaviour characterised by factor A, which involves high level discriminatory processes, originates at a cortical level, whilst that characterised by factor B, involving simple, unstructured movements, originates at a thalamic level. Evidence is discussed to show that this is compatible with known neurophysiology of thalamo-cortical relationships. On this view, behaviour characterised by factor B predominates when alpha rhythm is maximal, whereas when it is minimal behaviour characterised by factor A predominates. Naturally neither factor is a sole determinant of behaviour.

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# THE EPISTEMOLOGICAL FOUNDATIONS OF BEHAVIORISM. A CRITIQUE <sup>1</sup>

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## INTRODUCTION

The discussion concerning the problem of behaviorism has been going on for more than 30 years both in psychology and in philosophy, but still there is much confusion and little agreement. In so far as opinions on this issue tend to influence the actual research procedures and theoretical conceptions of psychologists, a clarification of the problems involved seems to be entirely relevant and rather important for theoretical psychology.

It is the aim of the present paper to provide an attempt at such a clarification, from the point of view of a psychologist. The formulations to be given are probably too vague to be of much value for the exact solution of complex problems in epistemology. They will, however, suggest certain points of view that seem to have occurred rather seldom in the philosophical discussion and an attempt will also be made to criticise explicitly some arguments put forth by philosophers like Carnap and Kaila. But, above all, it is our hope that the treatment to be given here will be of some value both for those psychologists who have been forced into the rat laboratory by some rigid sort of "behaviorism", and for those who continue to use all kinds of "introspective" methods, but who have an aching methodological conscience as a result of the pressure from their more "sophisticated" behavioristic colleagues.

The conception of science that is basic to the present treatment probably does not diverge on any major point from that which is commonly held in "logical empiricism" and related philosophical schools. (except with respect to the doctrine of "physicalism".)

In accordance with Carnap (7) a synthetic sentence P is regarded as meaningful if and only if it is confirmable, i.e. if it is known under what circumstances one would regard P or not-P as confirmed. (If "operationism" is interpreted as to mean that every sentence should be testable, i.e. that

<sup>1</sup> This paper was written and sent to the *Acta Psychologica* in 1951. It still adequately represents my main viewpoints, but there are many details in the argumentation that I would now prefer to have considerably more precise.

one should know how to produce the circumstances under which it could be confirmed, then the present point of view is "non-operational".)

Considering the essentially *social* nature of science, it is necessary to change the foregoing formulation, and the requirements for scientific meaningfulness may be stated thus: A synthetic sentence P is scientifically meaningful if and only if it is confirmable to every member of the scientific community, i.e. when it is known to every member of the scientific community under what circumstances he would regard P or not-P as confirmed, *and* every member describes the circumstances in the same way. (These are *ideal* requirements.)

Another shorter way of describing an essential characteristic of science is to say like Carnap (4) p. 448; "Die Wissenschaft ist das System der intersubjektiv gültigen Sätze."

Science is epistemologically dogmatic in the sense that it accepts only sentences that have the same place in the language of all scientists, or, stated in another way, science contains only "laws" or "principles" that are valid for all observers. Thus if two scientists describe the conditions of their respective observations in the same words and formulations, and if one of them asserts that the sentence P was confirmed (he observed that, which *to him* was meant by P), whereas the other scientist asserts that the sentence not-P was confirmed (he observed that, which *to him* was meant by not-P), the conclusion is never that there exist an irreducible difference in the sense that the "same" circumstances sometimes give rise to the phenomenon asserted by P, and sometimes not. Instead, the usual procedure in science is to try to "explain" the difference in one of two ways. First, one might try to find out whether the sentences and terms involved are used in the same way by the two scientists, i.e. whether these terms and sentences have the same place in their language. (Especially whether the terms and sentences involved are synonymous to the same other terms and sentences for both scientists, and even whether these other terms and sentences have the same synonymity relations etc.) If one can find no difference in the language of the scientists, the difference in observations is regarded as caused by undiscovered differences in experimental conditions, either extra- or intraorganismic, which means that the experiment is repeated with more and more extensive descriptions (more and more variables explored) until finally one arrives at a point where both scientists use the same terms in describing the conditions when P is confirmed, and when not-P is confirmed, which means that one has found an intersubjectively valid "law" or "invariance".

Implicit in the foregoing considerations has been the so-called principle

of "logical behaviorism", which, as we interpret it, states that there is no other meaning in the statement that two people observe (mean) the "same" thing (have the "same" experience) than that they use the same or synonymous formulations in describing it. (React to it with the same non-verbal behavior.) We will later try to show, that there is no connection between "logical behaviorism", as interpreted by us, and the philosophical doctrine of physicalism which is the background for psychological behaviorism. The position to be defended here is that whereas we accept the principle of logical behaviorism, we do accept as necessary neither psychological behaviorism, nor its philosophical background physicalism.

By "psychological behaviorism" we mean the methodological point of view in psychology that statements of subjects in an experiment are not to be accepted as reports of observations, but are to be regarded as "verbal behavior" and as "symptoms" of hypothetical states in the organism. Carnap (8) p. 57—58.

As exponents of this point of view we might mention besides philosophers like Carnap (4, 5, 6) and Kaila (11, 12, 13), especially the psychologists Bergmann (1) and Bergmann and Spence (2). They write:

(2) p. 4. "... the empiricist scientist should realize that his behavior, symbolic or otherwise, does not lie on the same methodological level as the responses of his subjects, and consequently that he should not in reporting the latter use any mentalistic terms which have not been introduced from a physicalistic meaning basis."

and further

"In studying his subjects, including their symbolic responses (object language), the behavior scientist himself uses a different language (pragmatic metalanguage)."

We will return later to an analysis of the implications of this point of view for psychological research.

By "physicalism" we intend to mean the same as such philosophers as Neurath, Carnap and Kaila. The term is very vague and is regarded by Ness as being useless in methodology (18) p. 54. We do not think, however, that the term is meaningless and we do not know of any plausible interpretations of the term as used by the above mentioned philosophers, to which our arguments do not apply.

In an earlier paper (4) Carnap writes: (in the formal idiom)

p. 441. "Die physikalische Sprache ist dadurch charakterisiert, dass ein Satz einfachster Form einer bestimmten Wertreihe der Koordinaten (drei Raum-, eine Zeitkoordinate) einen bestimmten Wert (oder Wertintervall) einer bestimmten Zustandsgrösse zuschreibt."

The corresponding formulation in the material idiom was:

p. 441. "Die physikalische Sprache ist dadurch charakterisiert, dass ein Satz einfachster Form die Beschaffenheit einer bestimmten Raum-Zeit-Stelle zu einer bestimmten Zeit quantitativ angibt."

Also a certain type of qualitative determinations were admitted:

p. 442. "Wir können qualitative Bestimmungen dann mit zur physikalischen Sprache rechnen, wenn sie als Bestimmungen physikalischer Zustände gedeutet werden, so dass z.B. die Sätze "Hier ist ziemlich kühl" und "Hier besteht eine Temperatur zwischen 5 und 10 C" als Sätze gleichen Sinnes genommen werden."

In a later paper Carnap seems to change his point of view slightly, although we are unable to see exactly what this change implies. He introduces the terms "thing-language" and "physical language".

p. 466 (7) "Let us give the name "*thing-language*" to that language which we use in every-day life in speaking about the perceptible things surrounding us. A sentence of the thing-language describes things by stating their observable properties or observable relations subsisting between them. What we have called observable predicates are predicates of the thing-language."

p. 466 (7). "Let us give the name "*physical language*" to that language which is used in physics. It contains the thing-language and, in addition, those terms of a scientific terminology which we need for a scientific description of the processes in inorganic nature."

p. 467 (7). "The so-called thesis of *Physicalism* asserts that every term of the language of science—including beside the physical language those sub-languages which are used in biology, in psychology, and in social science—is reducible to terms of the physical language."

But later on the same page Carnap continues:

p. 467 (7). "In former explanations of physicalism we used to refer to the physical language as a basis of the whole language of science. It now seems to me that what we really had in mind as such a basis was rather the thing-language, or even more narrowly, the observable predicates of the thing-language."

Because of the vagueness of the term thing-language it is difficult to know Carnap's exact position. There are, however, formulations as well in his own writings as in those of Neurath (16, 17), Kaila (11, 12) and Lenzen (15) which seem to indicate that the formulations quoted on p4 still are valid as giving the necessary and sufficient criteria for a sentence to belong to the physicalistic (thing-) language (when qualitative determinations are included) and we will therefore use "physicalism" in

accordance with Carnap's last definition. (It is still an open question whether sentences are *translatable* which is the opinion of Kaila (11) p. 233—241 and Wright (19) p. 127, or only *reducible* which was the opinion of Carnap, but this distinction is not important for our purposes.)

#### CARNAP'S DEFENSE OF THE PHYSICALISTIC DOCTRINE

Carnap seems to assert that the physicalistic language is the only possible scientific language, or more precisely that every possible scientific language can be reduced to the physicalistic language.

p. 463 (4) p. 463. "Jede mögliche andere Systemsprache ist also übersetzbar in die physikalische Sprache, kann gedeutet werden als Teilsprache der physikalischen Sprache in verändertem Gewand."

(Carnap would now probably to say "reducible") As applied to psychology the physicalistic thesis asserts:

(4) p. 450. "Die These besagt hier, dass alle Sätze der Psychologie/ (in the formal idiom)/ sich in die physikalische Sprache übersetzen lassen, und zwar sowohl die singulären als auch die generellen ("psychologische Gesetze"); oder was dasselbe bedeutet, dass die Definition jeder psychologischen Bestimmung auf physikalische Bestimmungen zurückführt. (in the material idiom) . . . von physischen Vorgängen sprechen (nämlich von den physischen Vorgängen am Körper und besonders am Zentralnervensystem des betr. Subjektes); sei es von bestimmten einzelnen Vorgängen, sei es generell von Vorgängen bestimmter Art eines bestimmten einzelnen Subjektes oder allgemein irgendwelcher Subjekte; m.a.W. jeder psychologische Begriff bedeutet eine bestimmte physikalische Beschaffenheit derartiger Körpervorgänge."

To this one might object that our present day knowledge about the central nervous system, the physiology of the retina etc. is too small to make a translation possible, i.e. we do not know what neurophysiological events occur during a psychological experiment. To this Carnap has the following answer:

p. 457 (4). "Allerdings ist das, was wir dabei über den Körperzustand des anderen Menschen wissen, gegenwärtig noch nicht formulierbar als Wertverteilung der in der Physik vorkommenden Zustandsgrößen; wohl aber formulierbar in anderen Ausdrücke der physikalischen Sprache, die gerade das treffen was wir brauchen. Bezeichnen wir etwa denjenigen Körperzustand als "rotsehend", der dadurch gekennzeichnet ist, dass auf die und die (physikalischen) Reize die und die (physikalischen) Reaktionen auftreten (z.B. Reiz: Wortklang "Was siehst du jetzt", Reaktion: Sprechbewegung "Rot"; Reiz Wortklang "Zeige auf dieser Farbtafel die soeben gesehene

Farbe", Reaktion: der Finger bewegt sich auf das und das Tafelfeld; usw."

We are unable to see that Carnap has escaped his difficulties by this. Because he has not really "translated" the sentences of the subject, but only decided not to accept them as sentences in the scientific language, but to regard them as verbal behavior referring to some state in the subject's body. Thus it seems to us as if Carnap has *not* demonstrated that the physicalistic language is (at present) universal, i.e. that all sentences are (at present) *translatable* (or *reducible*) into this language.

It seems as if Carnap makes the same methodological distinction between the scientist's and the subject's language as Bergmann and Spence (1, 2). And hence, by limiting the psychological language to statements about the behavior of organisms, the physicalistic thesis is safe, because then no sentences which Carnap accepts as belonging to science, are not physicalistic.

But regarded as a "proof", this is merely begging the question.

In a later paper Carnap is especially clear as to his point of view.

(8) p. 58. "We do not at all enter a discussion about the question whether or not there are kinds of events which can never have any behavioristic symptoms, and hence are knowable only by introspection. We have to do with psychological terms not with kinds of events. For any such term, say "Q" the psychological language contains a statement form applying that term, e.g. "The person . . . is at the time . . . in the state "Q"." Then the utterance by speaking or writing of the statement "I am now (or: I was yesterday) in the state Q is (under suitable circumstances, e.g. as to reliability, etc.) an observable symptom for the state Q. Hence there cannot be a term in the psychological language, taken as an intersubjective language for mutual communication, which designates a kind of state or event without any behavioristic symptom . . . Hence every term is reducible to those of the thing-language."

and finally on p. 59 he writes:

"Anger is not the same as the movements by which an angry organism reacts to the conditions in his environment, just as the state of being electrically charged is not the same as the process of attracting other bodies."

From these quotations it should be completely clear that the subject's statements are not by Carnap accepted as observational reports ("Proto-kollsätze") but as physical (behavioral) phenomena representing a state which is also represented by other physical (behavioral) phenomena. This is especially clear by the parallel drawn between "anger" and "electrical charge".



"Anger" is taken as a construct which is introduced by reduction sentences of the type  $A \supset (B \equiv \text{"Anger"})$ . But in these reduction sentences A and B must always be *physical* conditions or events. A sentence of the type  $A \supset (\text{"feeling angry"} \equiv \text{"Anger"})$  would thus not be accepted.

Now, what is really the methodological justification for Carnap's distinction between the subject's reports and the scientist's reports?

In everyday life one usually accepts a number of reports such as "I feel tired", "I see a red spot", "I dreamt about going to a cinema", as fully confirmable sentences, in the sense that one does know under what circumstances one would regard the sentences as confirmed or not. (Cfr. the criterion of meaningfulness given on p. 2.) And as used in this sense, the feeling of anger is *not* a predicate referring to a known physical thing.

We are completely unable to see what fundamental difference there might be between the confirmability of a sentence like "If somebody disagrees with me I feel angry" and "this pointer coincides with the number 45". (the question of reliability is not relevant here).

Carnap argues:

(5) p. 138. "Der Psychologe A schreibe etwa in sein Protokoll den Satz  $p_2$ ; "(Ich bin) jetzt aufgeregt". Eine frühere Überlegung hat gezeigt, dass die Auffassung, die Protokollsätze seien nicht physikalistisch zu deuten, sondern sprächen von nicht-physikalischem ("Psychischem", "Erlebnisinhalten", "Bewusstseinsdaten" oder dergl.), zunächst zu der Konsequenz führt, dass jeder Protokollsatz nur für das eigene Subjekt sinnvoll wäre. Der Protokollsatz  $p_2$  von A könnte nämlich, wenn nicht physikalistisch gedeutet, von einem anderen Subjekt B nicht nachgeprüft werden, wäre also für B sinnlos."

In order to demonstrate as clearly as possible the reasoning of Carnap we will finally quote the following:

"Die beiden analogen Sätze "In der ersten Schachtel sind Stahlfedern" und "In der zweiten Schachtel sind Stahlfedern" sind logisch und erkenntnistheoretisch von gleicher Art. Deshalb besteht hier der Analogieschluss zu Recht. Anders steht es mit den beiden Sätzen "Ich bin zornig" und "Der andere ist zornig." Wir halten den ersten für sinnvoll, den zweiten (wenn die physikalische Deutung ausgeschlossen wird) für sinnlos; der Einwendende der den zweiten Satz auch für sinnvoll hält, wird der Ansicht sein, dass der zweite von dem Behauptenden in völlig anderer Weise nachzuprüfen ist als der erste. So kommen wir zu dem gemeinsamen Ergebniss, dass der zweite Satz erkenntnistheoretisch von anderer Art ist als der erste. Die gleiche Sprachform ist logisch unrechtmässig. Sie täuscht uns eine Gleichartigkeit der beiden Sätze und damit eine Berechtigung des Analogieschlusses vor."



It seems to us as if Carnap (as we interpret him) is here fundamentally wrong. Because what we mean by the statement "In the other drawer are pens" is merely that *if* we observe under the conditions of looking into the other drawer, *then* we will observe pens. In the same way what we mean by the statement "the other man feels angry" is really that *if* we observe under the same conditions as the other man, we would feel angry. (Feel what we call angry.)

*The reason why Carnap regards the two cases as different, we believe is that he does not regard intraorganismic states as "observational conditions".*

This hypothesis of ours is strengthened by the following quotation:

(4) p. 122. "Aber — so wird man sagen — brauchen wir nicht die Aussagen unserer Mitmenschen, um die intersubjektive Wissenschaft aufzubauen? Wie dürftig würden Physik, Geographie, Geschichte, wenn ich mich auf die von mir selbst unmittelbar beobachteten Vorgänge beschränken müsste. Dies ist richtig; aber es besteht ein grundsätzlicher Unterschied zwischen einer Aussage des A über den geographischen Zustand Chinas oder über einen historischen Vorgang der Vergangenheit und einer Aussage des A über seinen gestrigen Zorn. Die Aussagen erster Art kann ich grundsätzlich nachprüfen durch Wahrnehmungssätze meines Protokolls, etwa über Wahrnehmungen von China selbst oder von einer Landkarte oder von historischen Dokumenten; die Aussage über den Zorn aber kann ich grundsätzlich nicht nachprüfen wenn im Sinne des Einwandes die physikalische Deutung ausgeschlossen wird."

If we know under what conditions "anger" is supposed to occur and how to produce them, statements about feeling anger are just as testable as statements about pens in drawers or about China. F. inst. the assertion to be tested might have been: "if you are hindered by somebody under circumstances a, b, c, . . . to reach a goal, then you will *feel* angry" this is perfectly testable (in principle) because anybody might put himself into those conditions and confirm the assertion or its negation.

A strictly parallel example is the case when an astronomer, looking through a telescope remarks "I see now one of Jupiter's moon". His friend who sits beside him can see no moon, but the statement is perfectly meaningful to him. It means to him that "*if* I observe under the conditions of my friend, i.e. *if* I look through the telescope, *then* I will see one of Jupiter's moons." It seems not to have occurred to Carnap that introspective reports are in no fundamental way different from this one, (at least we have not been able to find a single place where he discusses the problems from this point of view.) and the reason it did not occur to him

is probably that we have to day comparatively little knowledge about the intraorganismic conditions determining individual experiences and very few methods of testing introspective reports. (Another way of saying this is that introspective reports are usually confirmable, but very often not testable.)

In many cases one may directly try to create the required dynamic situation, one may apply learning procedures, elaborate instructions, drugs etc. Other cases are even more difficult, especially those where so-called personality factors and "constitutional factors" belong to the relevant experimental conditions. These are extremely difficult to manipulate. Even such situations do not, however, constitute an unsurmountable obstacle, because when active manipulation of variables is not feasible, one may still undertake passive registration of the Brunswik-type (3).

For instance a report of introspections made by a person with a certain set of relevant personality factors, can be tested by finding and studying other persons with the same distributions of the relevant factors. When considering that scientific laws always are of the general type  $(x) A(x) \supset B(x)$  it should be understood that this "passive" experimental design is in no way inferior to the "active" design as far as the confirming of general laws is concerned. Thus we are unable to conceive of any introspective report that should be "in principle not testable", and we conclude that for any useful meaning of "untestable" Carnap's assertion that observational sentences that are not given a physicalistic interpretation are untestable, is false.

A criticism of Carnap's position, to some extent similar to our own has been given by Duncker (9) who among other things writes:

(9) p. 165. "Ich fasse zusammen: so etwas wie Zorn als fremdpsychisches Erlebnis ist nicht vergleichbar mit jener von Carnap mit Recht beanstandeten Deutung von "Festigkeit" (z.B. einer Holzstütze). Eine derart metaphysisch interpretierte "Festigkeit" wäre allerdings eine "qualitas occulta". Zum Zorn hingegen hat, wer ihn in sich erlebt einen unmittelbaren Zugang. Es gibt also mindestens einen Standort, von wo "Zorn" (hier immer als inneres Erlebnis verstanden) keine *qualitas occulta* ist. Für "Festigkeit" dagegen fehlt ein entsprechender Standort."

We interpret Duncker to mean here that the construct "Zorn" has phenomenological "Erscheinungen" (modes of appearance) whereas the construct "Festigkeit" has not.

To this, however, Carnap (6) p. 184 answers:

"Analog Dunckers vorangegangener Überlegung könnte man aber

sagen: die Stütze selbst erlebt jene Festigkeit (so wie ich meine innere Festigkeit erlebe), sie hat zu dieser Festigkeit einen unmittelbaren Zugang."

It seems as if Carnap here has interpreted Duncker in some sort of metaphysical direction. The interpretation of Duncker that we regard as more plausible and which we would like to defend, is not touched by Carnap's argument, and runs like this: By the statement "the other man is angry" I mean that *if* I observe (or anybody else observes) under the "same conditions" as the other man, *then* I will feel angry. There is in this hypothesis no trace of assumptions as to what the other man "experiences" or what is in his "consciousness" (in any metaphysical sense). But his report is a signal as to what will happen to me under certain conditions.

"Die Stütze", however, gives no signals to me as to what will happen in classes of situations which I have not experienced before, and in so far the two cases are different.

As we interpret Carnap, there are a number of implications of his position, that to us seem absurd. We will here mention only two:

Suppose a man says: "When you go bicycling, you do not pay attention to your stabilizing movements." ("pay attention" *not* behavioristically defined.). As we interpret Carnap, he would declare this sentence as meaningless and "grundsätzlich nicht nachprüfbar". However, even if I cannot at present bicycle and thus cannot at present test the proposition, because my observational conditions are different from those of the other man, (I would probably pay very much attention if I tried to balance on the bicycle) if I go through the procedure of learning to bicycle, then I would be able to confirm or disconfirm the proposition.

Another implication of Carnap's point of view that seems to us rather absurd is this: Suppose you are a psychologist that is making an experiment with the well-known phi-phenomenon. (A horizontal and a vertical line are shown alternately in rapid succession.) Your subject reports: "I see a line moving rapidly from one of the positions to the other and back again."

You yourself also see the line moving rapidly back and forth, but according to our interpretation of Carnap, he would require that you report (in physicalistic terms) only the experimental set-up and the verbal behavior of your subject. You are not allowed to report that you, as a scientist, observed the phenomenon, and you are not allowed to regard the "verbal behavior" as a report of certain observations, which you yourself can and actually did confirm.

To our objections against Carnap's behavioristically introduced concepts as "red-seeing", "being aware", he might, however, answer the following:

(4) p. 459. "Stellen wir fest, dass irgendeine wissenschaftliche Bestimmung durch ihre Definition zurückgeht auf den und den Komplex anderer Bestimmungen und daher dasselbe bedeutet wie dieser, so wendet man uns immer wieder ein: "Aber wir *meinen* damit doch etwas anderes"; zeigen wir, dass zwei bestimmte Sätze auseinander ableitbar sind und daher denselben Gehalt haben, (in inhaltlicher Formulierung:) "dasselbe besagen", so bekommen wir immer wieder zu hören: "Aber wir *meinen* mit dem ersten doch etwas anderes als mit dem zweiten." Wir wissen, dass dieser Einwand auf der Verwechslung zwischen (logischen) Gehalt und Vorstellungsgehalt beruht (vgl. Carnap, *Scheinprobleme*.)"

We have some difficulty in following the above line of reasoning. Clearly the objections can be referred to as "Scheinprobleme" only *within Carnap's physicalistic language*. But the problem is not this, but whether one does accept and agree with Carnap's definitions of his terms. And a definition is always conventional. When considering that a definition gets into the scientific language, because of a widespread agreement as to what a term shall mean, it seems as if the vigorous objections by those who say "aber wir meinen damit doch etwas anderes" (in so far as they are scientists) can be taken as symptoms of disagreement and thus indicating that the definition might not be so good after all. Thus, it seems to us as if Carnap is wrong when asserting that our objections are due to "Scheinprobleme". (Although we are of course aware of the fact that his argument was based on the belief that he had proved that all intersubjective language is physicalistic.)

Our own beliefs, which are of course just as programmatic as Carnap's, may be summarized as follows: The future psychological system-language, the fundamentals of which we think already exist in the vernacular, is in so far a parallel to the physical language, as every sentence in the psychological language implies a conjunction of implications between observational sentences. *Some* of these observational sentences are equivalent with observational sentences occurring in the physical language, *some are not*.

Another way of stating this is to say that the psychological construct's (like f. inst. "Anger") have both physiological, behavioristic and phenomenological "modes of appearance" ("Erscheinungsweisen") (Cfr. Jørgensen (10) for an elaboration of this point of view.)

We may conclude this section then, by stating that, although the issues involved are very complex and the dangers of misinterpretation very

high, Carnap's arguments for the necessity of physicalism and consequently psychological behaviorism are not convincing to us. We will turn now to another prominent philosopher, Kaila, whose writings contain some additional arguments for the physicalistic doctrine.

#### KAILA'S DEFENSE OF PHYSICALISM

In his paper "Physikalismus und Phänomenalismus" Kaila has made an attempt to "prove" the following thesis:

(12) p. 90. "Es soll an einem wichtigen Einzelfall gezeigt werden dass der "Physikalismus" — in dem oben erläuterten Sinne recht verstanden<sup>1</sup> z.B. bezüglich der Psychologie kein unbegründeter "radikalistischer" Einfall ist, sondern sich zwangsläufig ergibt, wenn man das von mir so genannte "galileische Erkenntnisideal", das aller theoretisch höheren Wissenschaft zugrundeliegt, anerkennt."

And further:

(12) p. 90. "Dieses Ideal enthält vornehmlich die folgenden drei Punkte. Die Begriffsbildung muss hinreichend exakt sein, damit in jedem gegebenen Einzelfall die "prinzipielle" Möglichkeit vorliegt zu entscheiden ob der Begriff in diesem Fall zutrifft oder nicht. (diese in Wirklichkeit sehr summarische Formulierung genügt hier.) Diese Möglichkeit der Entscheidung ist jedoch offenbar nur dann gegeben wenn alle empirische Begriffe in dem früher angedeuteten Sinne auf gewisse empirische Grundprädikate zurückgehen. Zweitens muss die Theorienbildung hinreichend exakt sein, damit man klar ersehen kann was für Konsequenzen sich aus den aufgestellten Annahmen bezüglich der Erfahrung ergeben, wodurch erst die Theorien prüfbar, im günstigen Fall praktisch entscheidbar werden. Drittens soll die Begriffs- und Theorienbildung so vorgenommen werden, dass eine möglichst einheitliche Darstellung des einschlägigen Bereichs erreicht werden, d.h. dass dem für die theoretische Wissenschaft charakteristische "Invarianzprinzip" Genüge geleistet werden."

p. 91. "Es soll also gezeigt werden, dass wenn diese Postulate zugrunde gelegt werden, die Behaviorisierung der Psychologie des zwangsläufige Ergebnis ist."

In order to demonstrate this, Kaila first shows the necessity of the principle of logical behaviorism. (as we interpret the term, see p. 3.) He uses the example of a father who tries to find out what his son means by "red" and concludes:

p. 113. Er hat festgestellt, dass z.B. das Wort "rot" beim Sohn einen Zustand bezeichnet, der *in einem sehr weitreichenden Netz*

<sup>1</sup> We have not been able to find that he uses the term in any way different from the way it is used by Carnap.

von Reiz-Reaktionen-Zusammenhängen (von "Ordnungsbeziehungen") genau dieselbe begriffliche Stelle einnimmt wie die von ihm selbst als "rot" bezeichnete Qualität. Eben dieser begriffliche "Stellenwert", der allein feststellbar ist, ist die wissenschaftlich-theoretische Bedeutung des Wortes "rot". (als der Bezeichnung eines "Erlebnisses".)

We completely agree with this, only the last parenthesis seems a little ambiguous. If Kaila has meant that the word "rot" which occurs in the physicalistic language has some fundamentally *other type* of "meaning" we disagree. It seems to us as if the above considerations are valid for all scientific languages and that even in physics the intersubjective meaning of the undefined concepts (elementary predicates) only consists in their place in the language. ("begriffliche Stellenwert".)

Kaila continues:

p. 114. "Demnach kommen wir zu folgendem Ergebnis; Unabhängig davon, was man über die "Physikalisierung", den "Behaviorismus", das "Fremdseelische" usw. denkt, gelangt man in der theoretisch wissenschaftlichen Psychologie, die ein galileisches Erkenntnisideal aufstellt, zu einem Standpunkt der kein anderer ist als der Behaviorismus ("Gestalt-Behaviorismus")."

The thesis that we think Kaila has really proved by his arguments up to this point is what we have called "logical behaviorism" and which we regard as a perfectly sound principle. His use of the word "Behaviorismus", however, makes it probable that he believes he has also proved the principle of psychological behaviorism, and our suspicions are strengthened when at this point there follows a jump in the proof which we are completely unable to follow.

p. 114. "Demnach ist es eine Hauptaufgabe der theoretischen Psychologie, die "*behavioristischen Äquivalente*" aller psychologischen Ausdrücke — insofern sie wissenschaftlich bedeutungsvoll sind — herauszufinden, z.B. des "Rot-Erlebens" oder der "Einsicht". Diese "*behavioristische Äquivalente*" wollen wir dadurch etwas enger fassen, dass wir — in Übereinstimmung mit anscheinend zuverlässigen Erfahrungsbefunden — voraussetzen, jene Äquivalente seien ihrem Kern nach als *Gehirnzustände* aufzufassen."

What is the line of reasoning here? How can it follow from the statement that the only scientific meaning of the term "red" is its "begriffliche Stellenwert" (see above), that sentences containing the term "red" should be translated into sentences containing terms of the type "space-time coordinate"? Is not also the meaning of these words only their "begriffliche Stellenwert"? It seems as if Kaila has not distinguished between

on the one side "logical behaviorism" (as we use the term) and on the other side the completely independent principles of physicalism and psychological behaviorism.

That this is the case becomes quite clear when we find that Kaila has quite another concept "logical behaviorism" than we have—the *soundness of which he has, however, never proved*. He writes in "Den mänskliga kunskapen."

(11) p. 299. "Sålunda komma vi till *den logiska empirismens fjärde huvudet* eller den logiska behaviorismens princip. Denna lyder: phi-satser, vilka beskriva  $S_1$ :s erfarenhet, äro intersubjektivt sett innehållsekvivalenta med sådana f-satser, som framställa vissa kroppsliga tillstånd hos  $S_1$ ."

English translation: "Thus we arrive at the fourth main principle of logical empiricism, the principle of logical behaviorism. It sounds: Phi-sentences, that describe  $S_1$ :s experience are intersubjectively equivalent with such f-sentences, that describe certain bodily conditions of  $S_1$ ."

Kaila's f-sentences are physicalistic sentences, the phi-sentences are phenomenological ones.

It is rather easy to demonstrate the fallacy in Kaila's reasoning—it turns out to be the same as in the case of Carnap.

Kaila writes:

(11) p. 297. "...för att t.ex.  $S_2$  skall kunna bedöma phi-satser, som handla om  $S_1$ :s upplevelser, måste ur dessa satser följa något bestämt beträffande  $S_2$ :s erfarenhet. Annars kan  $S_2$  på intet sätt avgöra giltigheten av dessa satser, *då desamma ifall de icke uppfylla detta vilkor, äro utan realinnehåll för honom*. ... Emedan en dylik sats icke framställer något som rör den fysiska omgivningen av  $S_1$ :s kropp, och emedan den måste framställa något, som för  $S_2$  är "i princip iakttagbart", kan den ej framställa något annat än ett tillstånd i  $S_1$ :s kropp, närmast hans hjärna."

English translation: "...in order that f.inst.  $S_2$  be able to evaluate phi-sentences, that describe the experiences of  $S_1$ , these sentences must imply something definite with respect to  $S_2$ :s experience. If they do not, it is impossible for  $S_2$  to confirm or disconfirm these, *because, not fulfilling this requirement they have no real meaning to him*. ... Because such a sentence does not describe anything in the physical environment of  $S_1$ :s body, and because it has to describe something that is "in principle observable" to  $S_2$ , it cannot describe anything else than a state in  $S_1$ :s body, more specifically, in his brain."

Here seems to be involved a neglect of the fact that scientific sentences must have the form of an implication in order to be confirmable.

The report "I see red" is just as meaningless, taken in isolation, as the



report "I got as result of the measurement 2m/4 cm.". Science is not concerned with statements as "I observe so and so", but with statements of the type "Under conditions so and so, this will be followed by that". And the analysis of Kaila quoted above seems to indicate that he uses "observational conditions" in a very narrow sense. We think so because he asserts that either  $S_1$ 's report concerns something outside his body or something inside it, i.e. something that might be observed *independently of variations in intraorganismic observational conditions*. (by usual physical instrumentation.) This leads Kaila to believe that the report must concern something inside the body. If one, on the contrary, accepts intraorganismic observational conditions, Kaila's analysis becomes invalid, because then  $S_1$ 's report might also describe something not situated in physical space and *which can be tested by  $S_2$  only by bringing himself into the same psychological state as  $S_1$* . Earlier in the paper we have given a number of examples of such procedures changing the psychological conditions of the observer. (pp. 9, 10.)

Thus we again must conclude that the claims for the necessity of physicalism cannot be maintained. That there might be some arguments of a more practical nature favoring it, we do not, however, deny.

We are fully aware of the fact that in present day science, terms as "space-time coordinate" etc. have some outstanding properties (high reliability and intersubjectivity etc.) whereas many introspective reports contain terms that do not at present have these properties in the same degree. This does not, however, warrant the exclusion of the latter terms from science, and is certainly not relevant for the question of whether or not one can "prove" the necessity of the physicalistic doctrine.

Kaila could of course maintain as an empirical hypothesis that only the physicalistic terms have a *sufficiently* high degree of intersubjectivity to satisfy the two first points of his "galileische Erkenntnisideal" (See p. 14) but this might lead to the rejection of this "Erkenntnisideal" by many scientists. Furthermore many psychologists would reply that the classification of psychological phenomena implied by the physicalistic concepts, might do violence to the third point in the "Erkenntnisideal", namely the requirement of invariance, in the sense of that the uniformity and simplicity of theoretical explanations would suffer. Example: The difference in simplicity between a (psychological) theory of color vision in which *behavioristic* concepts "red-seeing", "blue-seeing" etc. (Cfr. the quotation from Carnap on p. 6) were introduced, and a theory where "red", "blue", "green" etc. were taken as undefined concepts in the scientific language. We do not here discuss the question which theory



would *actually* be simpler. We merely want to point out the futility of trying to "prove" that physicalism is *the* language of science and that psychology "must be" or "really is" behavioristic.

So far we have not found the arguments for the necessity of behaviorism very convincing. There is, however, one more type of argument, which has been presented by Kaila in the paper "Logik und Psychophysik" (13) where he tries to show that introspective (non-physicalistic) descriptions are mostly not "real" descriptions, but only "semantic" ones.

(13) p. 109. "Eine uneigentliche Beschreibung, die eine Sache durch irgendeine "Bedeutung", die die Sache hat oder haben kann, kennzeichnet, wollen wir als eine "*semantische Beschreibung*" bezeichnen. Die phänomenologische "Analyse" und "Deskription" ist im wesentlichen eine uneigentliche semantische Beschreibung. In der Wahrnehmungsphänomenologie etwa sind die Daten oft nur durch irgendeine "Bedeutung" charakterisierbar, die sie als eventuelle Signale ("Erscheinungen") von physischen Sachverhalten haben können."

The problems involved here are very complex and the dangers of terminological confusion are imminent. Within the limited space available we can therefore only briefly indicate some of the reasons why we cannot accept Kaila's line of reasoning (as we interpret him) as a valid argument against phenomenological description and non-behavioristic psychology.

One of Kaila's arguments is that the phenomenological analysis leads to contradictions, because of the structure of our language and must therefore be given up. (The surface of a table has at the same time the "Gestalt-qualität" of "plainness" and the "Gestaltqualität" of "unevenness" (not being smooth). The structure of our language requires that the "Gestalt-qualität" either be a "thing" or a "property", so runs the argument. But if it is a "property" of the table this leads to a contradiction because a "thing" cannot have two incompatible "properties". Or expressed in symbolic logic: The expression  $A(x)$  and  $\neg A(x)$  is a contradiction. The same is true if the phenomenon is regarded as a "thing", because it will have incompatible properties. In so far as the analysis of Kaila is correct (of which we are not sure) and it is true that the phenomenological analysis leads to certain linguistic difficulties and in so far as the phenomena described in the "phenomenological description" really exist (which even Kaila should agree with, because he would hardly have written a whole paper in the language without presupposing some degree of communication.) the only adequate thing to do would seem to be a reconstruction of the syntax of the language employed, rather than giving up the phenomenological analysis.

As to Kaila's main argument that phenomenological descriptions are semantical, the following considerations are relevant:

First, in any language there are a number of terms that cannot be defined, and that refer to something that cannot be further "described". To the extent that terms in phenomenological "descriptions" are of this nature but can nevertheless be used in establishing intersubjectively valid laws one cannot demand that they be further described—in any case the demand for description must stop at some point. (thus if one can establish intersubjectively valid laws for the occurrence of the quality of "roundness" (of a circle, a sphere, a melody, a man, a movement etc.) the fact that it cannot be given a "real" description is of no consequence.)

Second; we are unable to see what objections can be raised against the use in psychology (as a strict science) of such sentences as "P expects X to happen", "P now thinks of X", "P now intends X", "R to P means X" etc. where "thinks", "intends", "means" etc. are *not* given purely behavioristic definitions, or more precisely, where constructs like "expectancy", "thought" etc. have besides their physiological and behavioristic modes of appearance ("Erscheinungen") also phenomenological ones. (10) Kaila himself talks about extra- and intra-present "intendieren", and we cannot see that he has given any convincing reasons for why terms referring to these phenomena should not be accepted in a "strict" science. (he himself uses the terms, although he might assert that he is then not talking "strict science".) Whether such terms will eventually be used in scientific psychology seems to be entirely an empirical question.

Finally we would like to point out that there seems to be some confusion as to the task of introspective psychology. In our opinion its task is no more to describe the "act" of experiencing, thinking, dreaming, etc., than it is the task of the physicist to describe his "act" of looking at the spectroscope or his "act" of measuring a length etc. The task of introspective psychology is to describe *what* is "thought of", "dreamt of", "perceived" etc. and although Kaila's arguments might still apply, it is believed to be of value to have made the difference clear.

We conclude that, although we have not presented a complete and precise discussion of Kaila's paper and although we have had a feeling that there might exist some evil terminological confusion, Kaila's arguments against phenomenological descriptions have not convinced us.

Although there are still many difficult psychological and philosophical problems involved, we cannot see that it has been demonstrated that introspective (phenomenological) reports are "meaningless", in the sense not-confirmable, and even less we can see how there can be given any

reason whatsoever for excluding a class of phenomena from scientific investigation. Or, as formulated by Ness (18) p. 7.

"There are no psychological questions which, after having been made sufficiently unambiguous, defy scientific treatment, and there is especially no kind of privacy or immediacy of observations which can permanently exclude such treatment. A hypothesis of permanent exclusion would have to be built on a confirmed theory of scientific method, from which such an exclusion could be predictable. There is, however, no such theory."

It seems to us as if this argument of Ness applies to Kaila's and Carnap's position in so far as they propose that *not the phenomena referred to*, but *the reports about them* are to make up the subject matter of psychology.

We have tried here to give an epistemological critique of the behavioristic point of view (especially in so far as it is asserted to be the only possible one).

We have not done it because of any particular belief in the convincingness and fruitfulness of philosophical analysis for psychological questions. We have done it merely because it is the only way to change the convictions of those psychologists who base their concrete research program on some sort of invalid epistemological analysis and who, we believe, exert a tremendous influence on the direction of present day research and on the training of students.

The conceptual structure and methodology of the future science of psychology should not and cannot be restricted in advance by philosophical considerations—it will slowly grow out of experimental research on concrete special problems, and the constant empirical checking will ensure the soundness of procedures and theoretical superstructure. Whether or not one today accepts scientific terms that cannot be reduced to the physicalistic language (and it is the thesis of this paper that such terms exist) is entirely a matter of practical usefulness.

#### SUMMARY

The present paper is an attempt to demonstrate the futility of trying to exclude in advance and on the basis of epistemological analysis, certain basic terms from the language of scientific psychology, or, what amounts to the same, to exclude certain phenomena from the field of psychological investigation. More specifically we refer to the point of view commonly called behaviorism, which we have in this paper called 'psychological behaviorism' and which we define in the following way:

By "psychological behaviorism" is here meant "the position that statements of subjects in an experiment are not to be accepted as reports of observations, but are to be regarded as "verbal behavior" and as "symptoms" of hypothetical states in the organism."

It is tried to show that this point of view has *no epistemological justification* and that the "proofs" for it given by philosophers as Carnap and Kaila are fallacious, partly because they include among their premises the false doctrine of "physicalism" and partly because they do not distinguish clearly between what we have called "psychological behaviorism" (a methodological principle in psychology) and the perfectly sound but quite different principle of "logical behaviorism" (a metascientific principle).

By "physicalism" is here meant "the thesis that asserts that every term of the language of science (including that of psychology) is reducible (or translatable) to terms of the physical or thing-language." In the paper we try to show that this thesis is wrong.

By "logical behaviorism" is here meant "the principle that asserts that there is no other scientific meaning in the statement that two people observe the "same" thing (have the "same" experience) than that they use the same or synonymous formulations in describing it." This principle is regarded as perfectly valid, but also as having nothing to do with "psychological behaviorism".

The programmatic guess is ventured that the future science of psychology will contain constructs ("things") that besides physiological and behavioristic modes of appearance ("Erscheinungen") also will have phenomenological ones. It is concluded that the attempts to "prove" by epistemological considerations from a logical empiricism basis that psychology must be behavioristic ("the science of behavior") have not been successful and that what future selection of methods and concepts there might occur, will have to be guided exclusively by the research workers' experience and opinion as to their practical usefulness.

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## THE DIATONIC FOUNDATION OF HEARING

*A Note on Révész's two component theory of pitch perception*

BY

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Révész, in his new book on music psychology (1), recapitulates the fundamentals of his two component theory of pitch perception (2). He restates that the physical issue of frequency, as a property of the stimulus, is not associated with just one tonal attribute (pitch), but with *two* separate psychological correlates ("Merkmale"), namely *tone height* and *tone quality*. Height represents our sensory information with respect to rising and falling of frequencies, and materializes in a straight, continuous array. The sensation of quality implies a periodic recurrence of tones within the octaves and establishes the phenomenon of *octave periodicity*. To prove the duplicity of the pitch attributes, Révész refers to "factual separableness" of the two pitch-components as follows:

1. In the low and high extremes of the musical range, the quality of the tones vanishes. The large audible frequency domain above the musical range (extending from 4000 cps to about 16,000 cps.) actually represents a strictly unidirectional continuum; only rising and falling tone-heights can be sensed without "quality"-attributes.

2. Within the musical range, two distinct types of pitch-discrimination thresholds can be specified. The first reflects our differential sensitivity with respect to tone height, the second accounts for quality discrimination. Namely, one can detect the quality difference between two adjacent frequencies without detecting which was higher or, respectively, lower.

3. Révész describes a particular case of paracusis qualitatis. In the paracusis range (which extended over a fifth interval), the subject sensed height differences but heard every note as a *g#*. The subject had genuine absolute pitch.

4. Some music instruments (e.g. the drum) create sounds which can be sensed only by height, i.e. without quality. Noises can have similar properties.

Révész illustrated the duplex pitch properties of the musical tone system in the form of an ascending spiral. From the separableness of the two

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components, Révész deduces the genesis of *absolute pitch*. One with absolute pitch envisages tone quality and, therefore, shows in his tonal identifications the systematic tendency to commit only semi-tone or octave errors. One with so-called "relative pitch" estimates tone height, and spreads his recognition errors over a certain region centered about the test tone. Since this error spread is bounded, he with relative pitch ought also to be attributed a limited amount of absolute pitch sense. This is classified by Révész as "regional absolute pitch."

The two component theory, applied to musical intervals, leads to the suggested distinction between pitch distance and intervallic quality. Distance (denoting what is conventionally termed a musical interval) reflects the recognition of height difference. When the judgement is centered on the qualities of the two tones, this second type of pitch comparison leads to the concept of "intervallic quality." E.g. a perfect fifth and its inversion (a perfect fourth) represent identical intervallic quality.

## 2. *Historical background*

The most essential part of Révész's clarifications pertains to octave periodicity in the tone system. Révész's fourty years incessant emphasis on the octave points toward a necessity to incorporate the issues of octave periodicity into the physiological explanation of hearing. A brief historical survey elucidates Révész's importance.

Helmholtz's orientation in the theory of hearing was consciously based on the diatonic properties of the pure tones, Clangs and chords. Generally neglected is that the "*Lehre von den Tonempfindungen als physiologische Grundlage für die Theorie der Musik*" devoted 9 chapters of 19 to clearcut diatonic issues in hearing, (intervals, scales, consonances and dissonances, rules of harmonization.) Helmholtz felt need for an octave explanation (3), which he based on the criterion of the maximum number of coinciding overtones. In the physiological sector, the said stimulus criterion is assumed to generate maximum similarities between resonance patterns on the basilar membrane; on the subjective side, the consequence would be the maximum similarity in the corresponding tonal sensation. (As noted by Révész, the Helmholtz-criterion holds for neighbouring octaves only; moreover octaves of pure tones are also sensed as octaves. The suggested criterion is therefore invalid.)

The mechanistic theories of hearing, following Helmholtz (e.g. wave and pattern theories), could not offer new octave explanation, and since the arising difficulties were unsurmountable, the issue of the physiological octaves was dropped. Contemporary models of hearing, based on

mechanical and electric patterns, or on electric resonance, also cannot account for recurrent tone qualities. *Wever's* historical survey of the theories of hearing (4) does not imply one reference to further attempts to represent the issues of octave periodicity in the physiological sector.

Physiological irrelevance of the octave became the general conviction of both the neuro-physiologists of hearing and the psychoacousticians. They reject also the physiological anchorage of all other musical intervals, contest the reality of consonances and dissonances and usually relegate into the mental sectors the issues of absolute pitch versus relative pitch. All musical correlates of hearing are generally imputed to habituations and, in the neurophysiological sense, everyone is imputed absolute pitch. A musical test-subject insisting on musical cues to make his answers, is considered to be prejudiced by indoctrinations rather than to be of maximal reliability. A sharp dividing line is usually drawn between the natural state of non-musical hearing and the illusions based on the influenced state of musical hearing. In our day, any attempt to rehabilitate the physiological octave and the diatonic foundation of hearing sounds *a priori* unscientific.

Révész's book reverts back to the diatonic foundation of hearing and suggests a critical survey of the current anti-diatonic orientations.

### 3. *Lorenz*

The question whether the diatonic tone system, from the point of view of Psychophysics, qualifies as a valid scale was first challenged by Lorenz in 1891 (5). He based his doubts on the auditory system's uncertainties manifested in the ability of "halving" intervals. As a matter of fact, only certain specific intervals (e.g. an augmented fifth) lend themselves to bisections into equal and diatonically meaningful parts, (in this example, two major thirds). But even the trained musician becomes uncertain in regard to other musical intervals (e.g. a perfect fifth), and non-musical intervals in general. Can a scale, which does not allow for uniform security in fractionations of equal sense distances, be recognized as a psychophysically "valid" scale?

For centuries, the thesis, stating equal frequency ratios perceived as equal intervals (denoting "equal sense distances" in the terminology of psychophysicists), was accepted axiomatically. This thesis was also corroborated by the Weber-Fechner Law. A corollary could be derived from the Axiom: The half-point must coincide with the geometrical mean of the frequencies. Through its corollary Lorenz wished to test the validity of the Axiom. He studied equisection judgements with respect to arbitrarily chosen intervals and reported that, in the general case, equisections tended



to deviate from the musical criterion of equal frequency ratios and shift toward the arithmetic mean. Wundt and Titchener approved these findings as opposed to the views of the music psychologist Stumpf (6). In the eyes of the "positivists", the "validity" of the musical tone system seemed to be affected.

Resorting to up to date methods of stimulus control, Pratt repeated Lorenz's experiment. All interval equi-sections, within one octave, reconfirmed the Pythagorean Axiom (7). Furthermore, statistical evaluation of 9700 judgements collected within the range of three octaves (8), rejected the hypothesis of any equal-sense distance criterion different from the musical criterion. To this can be added that one ought not assume the subjective pitch scale dogmatically as representing an arithmetic continuum which lends itself to precise fractionations.

#### 4. *Contemporary theories of hearing*

The new trend in the theory of hearing inaugurated by Fletcher in 1929 (9), attributes auditory information to various spatial and temporal patterns which materialize on the membrane and in the auditory nerve and are partly mechanical, partly electrical in their nature. To explain merely the transduction of frequency and intensity information, the underlying structures can be easily imputed some sort of stereotype responsiveness which could account for the genesis of pitch and loudness. But auditory patterns, even when conveyed by resonators, are quality-deaf. To ascribe such patterns qualitative discriminations, so as to account also for recurrent tone quality, musical intervals, consonances and dissonances, could only lead to unsolvable involvements. *The revised theories of hearing had no alternative but to discard the diatonic aspects of hearing.* Moreover, in all these new hearing models absolute pitch standard grew out to be automatically implied; the response to a given tone had to be described in the form of an invariant pattern allowing, for the purpose of covering differential discrimination errors, only for slight spreads.

In the introduction to his first paper, Fletcher enumerated ten basic auditory issues which were expected to be covered by the space-time pattern theory of hearing. Two correlates of doubtful validity, usually invoked by psychologists without musical background, ("volume" and "brightness"), were actually included in Fletcher's list which called for explanation in the physiological sector. Analysis of complex tones into their components was stated without reference to the implications of consonances versus dissonances; the issues of octave periodicity and musical interval-sense associated with diatonic frequency ratios, were not

recognized; non-homogeneous treatment by the auditory mechanism of the high range (4000—16,000 cps) with the musical range (40—4000 cps) was bypassed. The issues of absolute pitch versus relative pitch, and internal pitch were not mentioned.

Fletcher's historical information analysis of the basic aspects of hearing came also to be accepted as a guide for research programs in Psychoacoustics founded on the electro-acoustical stimulus control of pure tones. In the past 25 years experimental work was expanded only with the study of musically irrelevant stimulus-artifacts processed in the psychoacoustical laboratory. (E.g. perception of pulses, bursts of noise, masking effects, interaural phase, binaural clicks, auditory fatigue and the like.<sup>1</sup>) Current text books on experimental psychology do not contain a reference to octave periodicity in the tone system, musical interval sense, Pythagorean ratios, and facts associated with absolute and relative pitch (10). For the explanation of hearing, speech perception is usually considered more crucial than musical information.<sup>2</sup>

#### 5. *Schemes to reform the musical tone system*

Certain music aestheticians blamed the diatonic tone system for the deadlock in contemporary music. The composer Busoni (11) anticipated the possibility of quarter tone music. Isolated attempts were made by builders of music instruments and composers to include quarter tones for the purposes of richer ornamental effects ("hyperchromatism") and even as equal righted tones in melody.<sup>3</sup> Révész analyzed particular schemes by Werner and Schole (12). In 1925 Werner suggested subdividing the interval of a third into micro-intervals separated by 5 vibrations per sec. steps. He claimed to have revealed possibilities of micro-melodies and micro-harmonies, but failed to evidence our auditory system to be psychophysically reliable in recognizing, analyzing and reproducing such micro-structures. For the same reasons, all other attempts envisaging the use of interval separations significantly different from the diatonic, also failed. Schole suggested in 1934 the extension of the musical tone system above 4000 cps. and claimed that with sufficient training, musical functions could

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<sup>1</sup> See the studies of W. R. Garner, J. C. R. Licklider, J. Zwillocki, G. A. Miller, J. D. Harris, I. Pollock, I. Hirsh etc.

<sup>2</sup> See the Progress Reports of the psychoacoustical laboratories at Harvard University and Massachusetts Institute of Technology.

<sup>3</sup> W. Moellendorf constructed a *bichromatic* harmonium; the firm Grottrian-Steinway built a quarter-tone piano. R. H. Stein, E. Bloch, Alois and Karl Haba composed music in quarter tones.

be actually introduced into this high frequency range. Révész's thorough analysis of the Psychophysics of the high tones reveals, that due to inherent lack of tone quality, the possibility of musical functions is completely ruled out in this high frequency range. To this can be added that modern electric instruments (e.g. the electric organ) could actually encompass reliable generation of all fundamentals in the entire audible range extended to 16,000 cps. The breakdown of musical functions must be imputed to the auditory system's limited resolution power.

#### 6. *The "mel"-scale*

Stevens, Volkman and Newman (13) sharing Lorenz's skepticism, attempted to establish the "true", "psychophysical pitch scale". In the conceptual framework of Mathematical Psychology (14), these authors presume measurability of our sensations, in all sense modalities, in terms of "ordinal", "extensive" or "intensive" scales. They referred pitch to the third category which, constituting essentially a ratio-scale, calls for numerical assignments attached to pitches. Since by definition, an intensive scale is one-dimensional and unidirectional, it could not allow for recurrent tone qualities.

To calibrate the subjective pitch continuum in terms of numerals, two experiments, based on bisections and fractionations, were devised. By the majority of psychoacousticians (10) the results are accepted as valid. We will briefly survey the shortcomings of this experiments and their evaluation.

1. In the first experiment, the invalid stimulus criterion of bisecting single pitches (i.e. without reference to a lower tone) was used.

2. The revised experiment was based on fractionations of pitch distances in four equal sense distances. The stimulus pairs were chosen in the range of 40—12,000 cps. Such amalgamation of the musical range (40—4000 cps) with the higher range (4000—12,000 cps) is inadmissible since with respect to all pitch operations (differential pitch discrimination, octave periodicity, intervallic operations, absolute pitch judgements), the two ranges are significantly non-homogeneous. Even for the person with absolute pitch, identifiability of the tones in the high range (4000—12,000 cps) is ruled out. It is apriori futile to expect statistically reliable intervallic operations (bisections, fractionations and the like) when both or one of the tones is above 4000 cps.

3. "Intra-observer" variability was appraised in term of "percentages of average deviations", (5.4—13.8%). Measurements can be validated in terms of their standard errors only. Since the tabulated averages ("half-

pitch" distances) were computed from but five settings each, standard errors and confidence levels should have been evaluated in terms of the "t"-test. On the basis of the reported data, one cannot attach a confidence level to the tabulated values.

4. "Inter-observer variability" was also reported in terms of "percentages of average deviation", (9.9%).

5. To base a psychophysical scale on inter-observer variability, i.e. group testing, raises serious concerns; a psychophysical scale must essentially rest on its intra-observer reliability. Mankind's intra-observer reliability in the diatonic system is manifest in the individual when registering simple diatonic tunes.

6. The basic curve which comprehends the "fundamental relation of subjective pitch to frequency" and is plotted in terms of the subjective "pitch-units" called "mels", was fitted with visual methods, without resorting to statistical procedures. Ten test subjects (including the experimenters) furnished the total of 450 judgements in a continuum from 40 cps to 12,000 cps.

7. Most likely, no psychoacoustical laboratory duplicated the described test.

All conclusions derivated or derivable from the pitch-scale are inevitably paradoxical. E.g. the authors stated that the diatonic intervals (octaves, perfect fifths) have different subjective sizes in different ranges. If this be true, transposition of musical tunes could not be sensed as invariances. Inasmuch as tunes transposed at a chosen diatonic interval are sensed by everyone as invariant, the authors need explain the genesis of such diatonic illusions. Are we actually listening to diatonic music with an absolute pitch sense anchored in "mel"-s and then rapidly transforming the "psychophysical magnitudes" of the pitches from the mel-system into the indoctrinated diatonic? Finally, if the diatonic tone system is merely an artifact based on habituations, do the authors pretend that arbitrary tone systems, other than the diatonic, could also be reliably trained?

#### *Note*

The immediate precedence, which might have served as an incentive for the "mel"-scale experiment, deserves particular attention. In 1933, a ratio-scale for the measurement of subjective loudness in the numerical units of *sones* was proposed by Fletcher and Munson (15). One method of calibration was based on the assumption that "two ears hear double loud." A second procedure used the "halving of loudnesses." The loudness function was calculated from group-medians. The authors reported that the two procedures yielded identical results, i.e. within observational error.

Statistical testing of intra- and inter-observer reliability did not appear in the published data. That group evaluation of the test judgements does not merely reflect public opinions of the proposed criterion, but can support and validate a psychophysical scale, was presumed by the authors. Licklider based on the sone-scale his three dimensional consideration of the field of audition. (10) After the lapse of two decades even psychoacousticians, favouring numerical measurement of loudness, "radically" disagree (16) on the actual shape of the "loudness-function" and its methods. If loudness would lend itself to numerical assignments and warrant intra-observer reliability, composers and performing musicians would adopt a numerical scale. Loudness is actually sensed in the frame of a level scale which extends from piano-pianissimo through forte-fortissimo, reaching the level of pain. To quantify loudness, only a contour-map of level curves is accessible for measurement. Finally, telephone companies recognized that binaural summation of loudness is of little significance and therefore discarded the second earphone. Two ears do not hear double so loud.

### 7. *Diatonic outlooks*

Partially under Révész's direct influence, isolated approaches were made to revalidate the diatonic correlates in the psychophysical and physiological sectors of hearing.

Bachem (17), accepting the principle of duplex pitch attributes and following in essence Ewald's "Schallbilder"-theory, identified the issues of tone-height and tone-chroma (a new term used for "quality"), by formations on the basilar membrane's vibratory patterns. The overall-patterns, characterized by certain spatial shifts, would account for tone-height, the sharper details for "chroma". The auditory system of a person with absolute pitch envisages the sharp details. At the end of the basilar membrane the vibratory patterns are the least precise; this accounts for "chroma limitation" above 4000 cps. To illustrate the two kinds of orientation in the frequency domain (and possibly along the basilar membrane), Bachem devised the slide rule analogy. Tone height would correspond to the "characteristic" of a number; chroma to the "mantissa". In the respective number domains mantissae are repetitive. One familiar with the slide rule (corresponding in the analogy to a person with absolute pitch), shows the systematic tendency to commit errors in identifying the "characteristics", (an illustration for the octave errors); meanwhile, he determines the mantissae correctly. (Analogy for the correct recognition of "chroma".) Bachem's last study transferred the issue of absolute pitch into the central sectors of hearing.

The physiological foundation of the tone system's octave-subdivision was recognized by Jeffress in an isolated remark. He assumed the cochlea to act as a "coarse screen in keeping frequencies an octave apart from

affecting the same synapse" (18). (A second and stricter type of frequency-place assignments is said to be achieved in the higher neural sectors.)

Licklider attempted to reconcile the issues of duplex pitch attributes with the quality-deaf layout of the electric resonance theories (19). He assumes that the peripheral ear-resonators are broadly tuned and their resonance accounts for the perception of height. Allegedly, a sharper analysis of the temporal patterns of the neural firings, in the form of their autocorrelational analysis, could resolve the quality of the tones. In this computer-analog model quality is reduced to the mental survey of temporal patterns; the latter recalls Leibnitz's concept of "silent computation by the soul" in connection with the frequency ratios of Consonances. The fact that universal absolute pitch is automatically implied, invalidates Licklider's model of duplex pitch perception.

### 8. *The diatonic foundation of hearing*

Révész's two component theory confirms the diatonic foundation of our hearing mechanism.<sup>4</sup> Révész's dictum stating that "the octave is the Alpha and Omega of the ear" ought to be accepted without reserve by all neurophysiologists and psychoacousticians. Some 2500 years of musical experience verifies universal acceptance of qualitative identity as associated with the octave, and also equality of the octave interval's subjective size over the entire musical range. When listening to tunes transferred to another octave or tunes in parallel octaves, and sensing their perfect qualitative identity, even the passive listener without musical training affirms the octave. Without psychophysically inbuilt octave-sense no ensemble music would be feasible, and reliable intonation would constitute a miracle. Therefore, even without invoking any "controlled test" with its "statistical evaluation", the psychophysicist with musical outlook can venture to declare that mankind's psychophysical reliability, in the production and detection of octaves, is hyperfine. No "Weber-fraction" ( $\Delta s/s$ , a universal measure for differential sensitivity) in any other sense-modality, could compete with the precision of the octave sense quantified in terms of  $\Delta F/f$  (The capital letter in the numerator refers to a just noticeable deviation from the Pythagorean octave,  $F = 2f$ ). When in the psychoacoustical laboratory reliability on the musical octave was actually tested, the error spread was found to be within  $\pm 1\%$  limits (20). Within the domain of hearing, only the unison-sense, (as reflected in the conventional

<sup>4</sup> In the present connection the term "diatonic" is used in a comprehensive sense encompassing "just intonation", Pythagorean intervals and tempered ratios. The discrepancies involved are only of the order of *just noticeable differences*, (j. n. d.-s).

differential pitch discrimination test and measurable in terms of  $\frac{\Delta f}{f}$ ), competes with universal reliability in octaves. Conjectures concerning possible "octave-deafness" lack foundation. E.g. in all oriental tone-systems, even those using non-diatonic interval separations, include the octave as a subdivision of the musical range. Naturally, no experimental evidence is available to refute the skeptic's speculations regarding possible indoctrination of pseudo-octaves to naive subjects. Even the Oriental adult is octave-conscious. Only very young children could serve as naive subjects. For lack of controlled learning tests, the music teachers' findings must receive credit. The very young child becomes octave-conscious quickly and spontaneously. Octaves are adopted more rapidly than any other type of diatonic interval. By intuition the music teacher can verify the futility of any attempt to indoctrinate pseudo-octaves as such. These could not be enforced since they do not evoke equal subjective qualities. Conversely, octave-insensitivity is impossible without pathological irregularities in the pitch sense. (Paracusis qualitatis).

Révész's dictum can be extended to all diatonic intervals. Our auditory system is capable, in the frequency continuum, to anchor a certain selected set of reference points with hyperfine reliability. Though since the time of Pythagoras the mechanism of such selective anchoring is unexplained, denial of its psychophysical reality would contradict basic auditory experience. In either active or passive form, everyone accepts diatonic interval separations; mere recognition of a simple diatonic tune in transpositions reflects the invariance of the diatonic intervals. Admittedly, mankind's 2500 years experience reliability in musical intervals does not in the formal sense qualify as "a psychoacoustical test based on electro-acoustic stimulus control and a recognized psychophysical method." From the meritory point of view, the experience of 25 centuries compares favourably with the confidence levels of data obtainable in psychoacoustical laboratories. Intonation of a musical interval can be assimilated, in essence, to an "adjustment" in the adjustment test; interval-discrimination by a passive listener is the counterpart to a "forced judgement" in a detection test. The integrated experience of 2500 years constitutes the "statistical validation". The test result is the *diatonic potential inherent in the auditory mechanism*. Meanwhile, the psychoacoustician is not barred from retesting, with up-to-date methods and equipments, the discovery of Pythagoras regarding the specific treatment of certain frequency ratios by the auditory process. Similar research work could contribute to specification of error-spreads about different types of intervals; e.g. the major seventh is known to be a "more difficult" interval



than a perfect fifth. "Just", Pythagorean and tempered intervals could constitute further fields for experimental research.

Consonances built on diatonic intervals are often referred from the fields of Psychophysics, to Aesthetics. Component tones are distinct in diatonic chords only; this criterion is cognitive and not aesthetical. Diatonic chords can be displeasing, e.g. in certain contexts.

Regarding the assertion that with training arbitrary interval separations are accepted by the auditory system with the same reliability as the diatonic, there is evidence to the contrary in the field of keyboard tuning. The tempered system is but slightly different from the diatonic. Nevertheless, even the most experienced tuner is incapable to set a reliable "temperament" without resorting to the diatonic "cue", (which is the perfect fifth or the perfect fourth); in tuning the additional keys, he must use comparisons in octaves. Admittedly, he is incapable to register tempered intervals in his "pitch memory." Any attempt to tune "by pitch" (without diatonic cues), e.g. in tempered semi-tone steps, would inevitably collapse because of error cumulation. In turn, the hypothetical task of tuning "by pitch" in the diatonic tone-system, would be manageable, (though such hypothetical keyboard would not serve the ensemble of our 30 musical keys.)

The customary objection, based on the Oriental scales and music, has in the present connection to be refuted. Psychophysical reliability of the oriental "intervals" is more than doubtful. E.g. the alleged 21 interval separations in the Hindoo scale are divulged by the clergy to the exclusive cast of the musicians only. The frequency ratios were never publicized and music writing was never devised. Listening with the Western ear, oriental melodies sound uncontrolled, or as vaguely controlled chants. Many instances verify that people from the far East, even in adulthood, reorientate themselves easily in accepting the diatonic tone system; conversely there is no record of reliable reorientation of a Westerner. To eliminate the suspicion of a merely fictitious tone system, a controlled psychoacoustical test of oriental interval-separations was never tried. Is it possible to verify such intervals by means of psychometric curves? A similar reliability should not be presumed and especially not in the light of 2500 years diatonic experience.

Historical evidences also support the privileged character of diatonic interval-separations; different types of diatonic scales were discovered independently in different ages and geographical locations.

Skepticism in the psychophysical anchorage of absolute pitch versus relative pitch is equally unfounded. The differences are customarily



referred to mental sectors, "pitch memory", training, "behaviors" or the like (21), or degraded to represent merely methodological issues associated with the absolute pitch test (22). Absolute or non-absolute pitch perception is anchored in the primary sectors of the sensory process,<sup>5</sup> absolute pitch judgements do not involve memory scanning and do not follow the error-laws of memory. Absolute pitch is an all or none affair, i.e. indivisible with respect to all tones within the octave. Absolute pitch is indelible; infallible against deceptions; spontaneously manifest in early age; it cannot be acquired subsequently by training; it is independent from environmental influences and is inheritable. Révész (23) and Bachem (24) verified experimentally that the error spread in absolute identifications can be classified as a psychophysical error-function; the Révész-Bachem curves are psychometric curves. Psychophysical "reality" of relative pitch becomes most obvious in the current process of hearing. When listening to music, one with relative pitch merely follows musical intervals without precise information regarding the frequency standard. This information is not "resolved". To base the physiological explanation of hearing on a universal absolute pitch sense is therefore incorrect. The correct hearing model has to account for the essentially relativistic nature of auditory information.

## 9. *Conclusions*

Physiologists and psychoacousticians proposed various auditory phenomena which could reveal the crucial facts in hearing. This study, in accordance with Révész, contends that the pillars of our hearing sense are octave-periodicity, musical intervals, diatonic scales and consonances. These issues can be comprehended as the diatonic potential in hearing.

The phenomena of absolute pitch versus relative pitch which can be classified under the frequency standard of hearing, are no less crucial. Finally, we refer to the neglected facts of internal pitch implying reliable auditory operations without physical stimulus and revealing the role of the auditory cortex in the process of hearing.

The three fundamental manifestations, diatonic potential, frequency standard, and internal pitch are expected to lead to a single and uniform explanation. Nevertheless, different paths leading up to the same mountain peak do not always require the same efforts; the shortest path is always the steepest. In the Appendix of this study, the writer proposes a "short-cut" by identifying, in the light of information analysis of sinusoids, the

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<sup>5</sup> An extensive literature on absolute pitch verifies the issue of "immediacy". The writer of this study has genuine absolute pitch.

"transfer function" which can account for the reliability of hearing at a non-absolute frequency standard.

#### SUPPLEMENTARY NOTE <sup>6</sup>

The process of hearing materializes in the form of *optimum transfer*. The acoustical wave information is encoded by the Corti organ in time series (= neural firings) and then decoded, by scanning the acoustical area of the cortex, in the form of a running power spectrum. The decoding process observes the autonomous time reference of the cortex. *Asynchrony* of this "psychophysical clock" with physical time flow causes *drifting* of the frequency standard of hearing. "Absolute pitch" emerges if the "psychophysical clock" is fairly *synchroneous* with physical time flow. Octave periodicity is laid out in the cortical decoding matrix. Diatonic consonances represent maximally distinct cortical spectra, (highest autocorrelations) compatible with the scanning rhythm.

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# A NEW FLICKER APPARATUS FOR MEASURING INDIVIDUAL DIFFERENCES

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## 1. INTRODUCTION

The study of the flicker phenomenon is centuries old (Landis, 1951) and in recent years interest in it has increased. The question whether the flicker threshold can be regarded as a characteristic of the individual or not, is the subject of the present study.

Experimental evidence from similar attempts, published during the last two decades, is not conclusive, mainly because the objects of those investigations—and even the concepts of what was to be measured by the flicker test—differed widely (see surveys by Biesheuvel, 1938; L. W. Jones, 1939; Cattell, 1946; Schmidtke, 1951), and also because the flicker determinations, as shown by Landis (1951) and McNemar (1951), were not uniform, the conditions of measurement being either unsuitable or not fully specified. Therefore, contradictions between the results of different research workers occur at different levels; they are only partly due to differences in theoretical interpretations, but are to a large extent introduced by relevant, and often overlooked, differences of method. In spite of these defects, the experimental findings so far are most valuable in showing clearly that the flicker threshold is an extremely sensitive measure of different psychological states or typical differences between individuals. This being the case, it seems advisable to defer discussions of certain theoretical problems—e.g. whether flicker is a measure of “perseveration” or “rigidity”, of “fatigue” (Henry, 1942; Schmidtke, 1951) or “Egokym” (von Bracken, 1951, 1952)—until a method of flicker determination can be established producing a “flicker score” which is a proved personal characteristic of an individual at a given time, reliable enough to warrant theoretical interpretation.

It was the purpose of this investigation to concentrate on the methodological and technical part of the problem. A brief indication only will be given of how an attempt was made at discovering the significance of the flicker phenomenon.

## 2. METHODOLOGICAL DIFFICULTIES. INFLUENCES OF LEARNING AND EXPECTATION

A considerable amount of factual knowledge about determinants of flicker threshold has been furnished by previous experiments. This includes the relationship of flicker fusion to intensity, colour, size, shape, light : dark ratio of the flicker stimulus and to its location on the retina. Furthermore, it is known that fusion frequency is affected by the circumstances under which observations are made, such as the state of light or dark adaptation of the subject, illumination of the room, distance and duration of observation, age of the subject, time of day, effects of fatigue, hunger, smoking, and of different drugs or stimulants. Most of these factors can be held constant in an experiment, the aim of which is to measure individual differences. We could decide beforehand to use the same method of presenting the flicker stimulus, the same light : dark ratio of the intermittent light and the same external conditions for the whole experiment; we could also choose the intensity and shape of the light patch and keep these constant throughout. It is difficult to decide, just which conditions are to be kept constant, because other important influences remain—influences which have been brought to light by recent research, but not yet thoroughly investigated.

The possibility that practice and expectation may systematically influence the flicker threshold is a serious drawback in establishing fusion frequency as a personal constant. Using the *method of limits* which is, to the writer's knowledge, the only one to have been used so far, these factors cannot be eliminated and are extremely difficult to control. If a subject is shown a series of flicker stimuli regularly increasing or decreasing in frequency, he is bound to build up an expectation that at a certain point flicker will cease or begin. And the more practice he has in doing so, the more definite will he become about the point at which he expects the change. The results of experiments by Knox and by Schmidtke—and probably many others who did not state it explicitly—show that CFF is different when the subject is set for seeing flicker from when he is set for fusion (Knox, 1945); and that it is different too for series of stimuli presented in ascending or descending order (Knox, 1945; Schmidtke, 1951.) It seems very likely that expectation and "experiential inertia" are inherent in the method of limits, which must make this method especially subject to the influences of practice and learning (Knox, 1945; Misiak, 1948; Mundy-Castle and McKiever, 1949). The possibility of practice effects is even greater where the apparatus is not perfectly silent. The

hum of a motor changes with its speed; the usual electronic devices produce a noise which changes in pitch or quality with the frequency of the photic pulses. Even if such apparatus works silently, the quality of the light produced by electronic stroboscopes varies with the frequency of the light pulses (McNemar, 1951, pp. 14-15). Except perhaps in a few very recent studies, experimental procedures appear not to have prevented the subject from judging the point of fusion, consciously or unconsciously, by the pitch of the apparatus or by other qualitative differences.

The remarkably high test-retest reliability of flicker fusion, reported in some experimental studies (Misiak, 1948; Erlick and Landis, 1952; McNemar, 1951; Schmidtke, 1951), could well be an artefact of the method rather than a true reflection of the constancy of an individual's flicker threshold—particularly in view of contradictions in the results obtained (Mundy-Castle and McKiever, 1949; Misiak, 1948, and McNemar, 1951). Interpretation of results remains questionable as long as the possibility is not entirely excluded that the subject learned to respond to a certain combination of facts instead of responding to the flash rate of the stimulus, and only to this.

### 3. APPARATUS AND EXPERIMENTAL PROCEDURE

#### (1) *Apparatus*

In order to overcome these difficulties, our problem was to devise an apparatus which would enable flicker stimuli to be presented in a *random order* with respect to frequency, and which would *not give any extraneous clues* to the subjects.

This was achieved by a simple alteration of the usual episcotister and by combining it with a projector, the purpose of which is essentially to separate the apparatus from the point where the stimulus is to be observed. The main constituents of this apparatus are: a small projector, a series of metal discs around the circumference of which holes have been drilled at equal intervals, and a strong, electrically driven gramophone motor. The motor is mounted so that individual discs can be attached to its axle and be rotated between the lamp and the lens of the projector. Any ordinary 2" × 2" slide projector can be used, provided there is sufficient space for the discs to rotate and to be changed easily. (The Leitz projector "Prado", which was used in this study, is very suitable.) The most important part of the apparatus is the set of discs, 250 mm in diameter, each having holes drilled round a circumference of 100 mm radius. The holes vary in number from 25 to 48 and in diameter from

12.57 mm to 6.55 mm. The relative position of the parts can be seen from the schematic drawing, figure 1.

The light, produced by the 200 Watt projection lamp, passes through a condensor and through a small aperture (1 mm in diameter) in an opaque slide. It then passes through the lens ( $f=8.5$  cm; relative opening 1:2.5) and is projected as a circular light patch on a white screen in the observation room (see (3), below). Immediately behind the slide, the light beam is cut into light and dark phases by one of the rotating discs

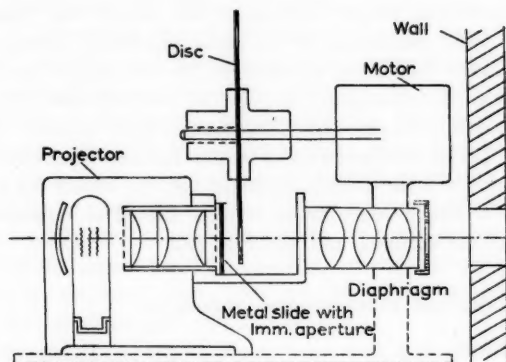


Figure 1. Scheme of flicker apparatus.

and its circle of holes. The motor driving the discs runs at a constant speed of *1 revolution per second*. Thus the number of holes in an individual disc corresponds directly to the flicker frequency, expressed in flashes per second.—It is important that the distance between slide and rotating disc is as short as possible and that slide and discs have dull, black surfaces. This helps to avoid reflections which would, in the dark observation room, create in the area surrounding the stimulus a flicker of the same frequency as that of the light spot itself.

If only alternating current (50 cycles/sec.) is available, a limit must be set to the range of flicker frequencies that are to be produced by this apparatus. Using this source of energy for the projector lamp and for room illumination, interference is produced at flicker rates of 50 flashes/sec. and more. As mentioned already by Schmidtke (1951, pp. 421–22) and as observed in preliminary experiments, the interference between light pulses of the stimulus and periods of the electric power system tends to make flicker reappear at high frequencies of about 60 fl/sec. But such



interference can be avoided by restricting the range of flicker frequencies below a maximum of 48 fl/sec. by adjustment of the stimulus brightness.

It is well known that fusion frequency is a function of stimulus intensity. Within middle ranges, it is approximately a linear function of the logarithm of light intensity (Bartley, 1941, Ch. VI, pp. 117 et seq.; Erlick and Landis, 1952, 380-81). That is, other things being equal, CFF drops when the brightness of the flickering light is reduced and vice versa. As the intensity of the light can be varied by different openings of a diaphragm attached to the projector lens, brightness being approximately proportional to the area of the diaphragm opening, it is possible to fix the apparent brightness of the light patch at an appropriate level. With the apparatus described here, a diaphragm of 7 mm diameter has to be used.

## (2) *Stimulus Conditions other than Intensity and Flicker Frequency*

(a) Light: dark ratio. An important factor is the light: dark ratio, that is the ratio between the duration of a single light flash and the interval of darkness between two consecutive flashes. The relationship between light: dark ratio and fusion frequency—intensity kept constant—seems to be a fairly complex one (Bartley, 1941, pp. 119-22).

The experimental findings are that, for the low intensities used in the present experiment, CFF tends to reach its maximum with light and dark periods of equal length. Since other investigators obtained satisfactory results with this ratio, and in order to avoid complications, it was decided to use a constant light: dark ratio of 1:1. In contrast to most electronic flicker devices, this ratio does not vary for the present apparatus when the frequency of the intermittent light is changed (cf. Erlick and Landis, 1952). The discs are constructed in such a way that the perforated circumference is divided into equal lengths, the diameter of each hole being equal to the distance between holes. For example, the circumference of the disc having 30 holes and producing 30 fl/sec. is divided into 60 equal parts, the single hole being

$$2 r \pi / 60 = 10.47 \text{ mm in diameter.}$$

The form of the light wave resulting from this arrangement, i.e. the curve of intensity of the intermittent light plotted against time, approximates very closely to that of the normal episcotister with sector shaped shutters (see figure 2). One difference is that the light is interrupted by the edges of holes of different diameters instead of by straight edges. This results in a slight reduction of the total intensity of the light phases,

particularly those of high frequencies. Since this deviation is subliminal and in no case exceeds 2 % of the theoretically correct brightness, it may be neglected here. Another difference concerns the relative length of the transition time from darkness to maximum brightness and vice versa. For the episcotister, producing the various flicker frequencies by different speeds of the revolving shutter, the ratio of the transition time to the time of light or dark phase is constant. For the present apparatus, the *absolute* time of transition is constant, namely 1.59 sigma, and its ratio to the time of light and dark phases varies with the duration of the latter and therefore with the frequency of the intermittent light. The duration of one light or dark phase, respectively, is

$$T_{\text{light}} = T_{\text{dark}} = \frac{1 \text{ second}}{2f},$$

where  $f$  is the number of light flashes per second. For the lowest frequency (25 fl./sec.) it is 20.00 sigma, for the highest (48 fl./sec.) 10.42 sigma.

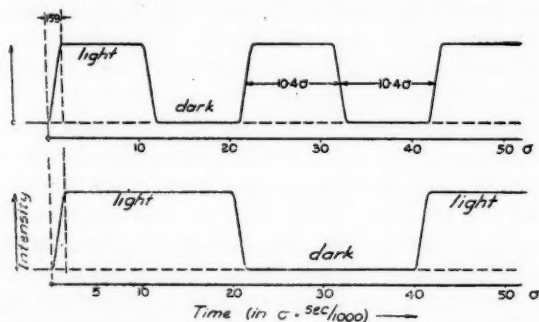


Figure 2. Form of light wave for lowest frequency = 25 fl./sec. (lower part) and highest frequency = 48 fl./sec. (upper part). Note that the time for the transition from dark to light and vice versa is the same (1.59 sigma) for both frequencies.

(b) Shape, size and colour of the target. From the technical and experimental point of view, the most convenient shape of the light patch would be a *circular* one. This can most easily be controlled with respect to size and location of the stimulated area on the retina. The flicker threshold differs from peripheral to foveal vision. Since unknown and uncontrolled individual differences are likely to be less in foveal vision alone than in foveal and peripheral vision, it seemed advisable at first to study the conditions of foveal vision only. The size of the circular

stimulus should, therefore, not exceed a *visual angle* of  $2^\circ$ , which corresponds in our case to 64 mm at an observation distance of 1.85 m ( $\approx 6$  feet), between the subject's eye and the screen. The distance of the projector lens ( $f = 8.5$  cm) from the screen has to be about 2.10 m ( $\approx 7$  feet), in order to project the 1 mm aperture of the slide in the correct size.

It was intended to compare the consistency of flicker determinations under different conditions of illumination of the test room. Scotopic vision—i.e. in the dark and with dark adapted eyes—is more or less "colour-blind". Needless to say, in these circumstances only *white light* should be used.

(c) Subjects; conditions of room illumination and stimulus field. When the experiment was started, it was not known under which conditions of illumination, intraindividual fluctuations would be relatively small in comparison with differences between individuals. The total group of subjects—all members of the staff of the National Institute for Personnel Research, Johannesburg—was divided randomly into three subgroups of 8 subjects each, and each of these groups was tested under systematically varied conditions. The mean age of the subjects was 25.96 years, S.D.  $\pm 7.52$  years, and the range from 18.25 to 46.5 years.

For *group I*, the experimental room was normally illuminated by a 150 Watt lamp, suspended from the ceiling in a sphere of white milk glass and hanging about 1.50 m ( $\approx 5$  feet) above the subject's left shoulder.

For *group II*, the room was darkened as completely as possible. Very little scattered light, coming in through ventilation openings, keyhole, and a little opening in the wall of the cubicle (see below) through which the stimulus was projected into the observation room, could just be noticed after some 10 or 15 minutes' dark adaption. No special control of the state of dark adaptation was made for this experimental condition.

For *group III*, a very dim illumination was effected by a Kodak darkroom lamp fitted with a 60 Watt bulb, the brightness of which was reduced by a filter of white drawing cardboard with a 7 mm hole in it. This lamp was fixed to the wall, 2.00 m ( $\approx 6\frac{1}{2}$  feet) to the left of the stimulus field, but so that its light was merely reflected from the white ceiling and could not dazzle the subject when he observed the stimulus. This diffuse light shone sideways on the screen and thus kept the brightness of the stimulus field at a very low level.—Dark adaptation was roughly controlled for this experimental group by starting the flicker determination always at that moment when the outlines of the screen could just be recognized and the position of its

centre be estimated. This took from 3 to 10 minutes, depending on the subject and his previous exposition to light.

The differences in room illumination determined the condition of the immediate surrounding of the stimulus, that is the screen of white paper ( $205 \times 300$  mm, long side vertical) on which the stimulus was projected. The screen was mounted on a light grey cardboard ( $350 \times 500$  mm) hanging on the grey wall opposite the subject, with its centre at the height of the subject's eyes. Thus the area of the screen appeared white on a moderately grey background, when the room was illuminated; the screen could be distinguished as a lighter rectangle from the darker background in the dimly-lit room; and nothing of it—except the fixation point—could be seen in the dark room.

For fixation, a small yellow spot in the middle of the screen, marking the centre of the stimulus, was painted with luminescent paint, so that it could be seen in the dark too. With the dim light of the Kodak lamp, the fixation point was not always recognizable, and the subject was then asked to fixate at the estimated midpoint of the screen.

### (3) *Experimental Procedure*

The flicker apparatus was operated from a cubicle built into the observation room. The partition was not sound-proof, but let no light through except the projected light of the stimulus. The experimenters could work in this cubicle with normal daylight, even when the subject was sitting in the dark. It was impossible for the subject to see anything of the experimenters' manipulations of the apparatus nor could he perceive what the experimenters' reactions to his own responses were. He could, however, hear the noise of discs being changed, and this changing was therefore done regularly after each presentation of a stimulus, regardless of whether a different frequency was actually used or the same one as before.

The subject sat on a chair opposite the screen, leaning back in a comfortable position. After being instructed as to what was required of him, the experimenter showed him an obvious flicker and an obviously steady light, beginning one day with the flicker, the following day with the steady light, and so on. The subject was then told that about 2 seconds after the experimenter had called "now", the light would appear on the screen in front of him for three seconds, and that he was to call to the tester "flicker" or "steady", as the case might be. The timing of the interval between warning and presentation of the stimulus and that of the duration of the stimulus exposition was done by simply counting the number of revolutions of the disc, one revolution being equal to one second. The

subject was allowed to look around during the pause between two flicker presentations, but was asked to keep his eyes and head steady during the few seconds of observation, looking at that time at the centre of the screen. He was asked to judge how the light appeared to him in the middle of the short observation period, especially in cases where he was doubtful.

After these instructions had been given, the subject was left alone. In the cubicle, the experimenter's assistant then picked out the first disc, beginning with a frequency somewhere near the average threshold. This was exposed to the subject and his response recorded. In the same way a number of discs were presented, giving different frequencies in a random order, each disc in the range of the threshold being shown four times. The procedure was continued until a certain lower frequency was responded to four times out of four as "flicker" (this was taken as the lower limit of the subject's responses), a certain higher one four times out of four as "steady" (upper limit), and all frequencies between these two limits were also responded to four times, but not uniformly. Normally, about 16 to 20 flicker stimuli had to be shown, until the limits were fixed; in some cases only 8 to 10, in other cases as many as 28 or more were necessary, depending on the subject's decisiveness and consistency. Once the limits and CFF were established, an immediate check on the speed of the motor was made by counting 60 revolutions and measuring the time by a reliable stop watch. In this way, even very slight changes of the speed of the motor—due to fluctuations of voltage and power periods—could be detected and, if necessary, corrected.<sup>1</sup> The actual fusion frequency of the subject was then determined by the formula:

$$\text{lower limit} + .5, + .25 \text{ for any "flicker" response} \\ \text{above the lower limit;}$$

or by the equivalent formula:

$$\text{upper limit} - .5, - .25 \text{ for any "steady" response} \\ \text{below the upper limit.}$$

This value of the subject's fusion frequency is eventually to be corrected by the factor  $\frac{60}{t}$ , where  $t$  is the time read off for 60 revolutions from the speed check.

<sup>1</sup> It may be mentioned here that a mere stroboscopic speed control of the revolving discs was not sufficient. The frequency of the power cycles was found to drop from 50 c/sec to 48 c/sec sometimes, particularly on cold mornings during winter.

TABLE I

(a) Group I (illuminated room):

Subject	1st. day	2nd. day	3rd. day	4th. day	5th. day	Mean	S.D.
1	41.5	39.2	39.2	39.0	38.5	39.5	1.2
2	41.8	43.0	41.2	40.3	39.0	41.1	1.5
3	42.2	40.8	42.8	43.6	43.8	42.6	1.2
4	39.6	40.0	38.5	39.0	39.9	39.4	.6
5	37.5	41.8	42.8	38.0	40.9	40.2	2.3
6	41.8	45.0	44.8	41.5	44.1	43.4	1.7
7	41.5	41.5	40.0	40.0	38.2	40.2	1.4
8	40.5	42.5	42.5	42.8	42.5	42.2	.9
Mean	40.8	41.7	41.5	40.5	40.9	41.1	(1.4)
S.D.	1.6	1.8	2.1	1.9	2.3	(3.4)	

(b) Group II (dark room):

Subject	1st. day	2nd. day	3rd. day	4th. day	5th. day	Mean	S.D.
1	35.5	36.6	37.9	37.2	38.2	37.1	1.1
2	40.0	41.3	39.2	40.2	39.6	40.1	.8
3	(38.5)	30.3	31.6	29.6	28.8	31.8	3.9 (.6)
4	33.7	32.3	30.2	29.7	29.9	31.2	1.8
5	38.1	38.0	37.4	38.5	39.0	38.2	.6
6	39.1	37.9	38.0	37.0	37.8	38.0	.8
7	39.2	40.5	40.0	40.9	41.5	40.4	.9
8	38.2	38.3	38.1	39.2	39.5	38.7	.6
Mean	37.8	36.9	36.6	36.5	36.8	36.9	(1.7)
S.D.	2.1	3.8	3.6	4.5	4.7	(7.9)	

(c) Group III (dimly-lit room):

Subject	1st. day	2nd. day	3rd. day	4th. day	5th. day	Mean	S.D.
1	36.7	34.3	35.0	34.8	32.9	34.7	1.4
2	37.3	39.8	39.2	39.7	39.3	39.1	1.0
3	38.8	40.8	40.1	42.0	43.6	41.1	1.8
4	39.2	40.4	40.8	44.6	46.0	42.2	2.9
5	37.5	37.9	38.6	39.0	39.9	38.6	.9
6	41.4	41.3	44.6	42.3	43.6	42.6	1.4
7	39.0	40.5	42.7	41.3	40.5	40.8	1.3
8	43.2	42.0	42.1	42.9	41.4	42.3	.7
Mean	39.1	39.6	40.4	40.8	40.9	40.2	(1.6)
S.D.	2.2	2.5	2.9	3.0	3.9	(5.9)	

Table I, a, b, c. CFF of three groups of 8 subjects, tested under different conditions of room illumination on 5 consecutive days.

#### 4. RESULTS AND DISCUSSION OF EXPERIMENTS

##### (1) *Ratio of Variances "between Individuals" and "within Individuals"*

The three groups, corresponding to the three illumination conditions (see part (2, c) of the previous section) and consisting of 8 subjects each, were tested on 5 consecutive working days between Wednesday of one week and Wednesday of the following week. Each subject had his (her) session always at the same time of day (between 9.00 a.m. and 12.30 p.m.), in order to avoid error owing to the influences of fatigue or similar factors (Schmidtke, 1951; and others).

The results of the 120 determinations of CFF with 24 subjects (table I) were subjected to an analysis of variance, and the components of the total variance separated according to the relevant aspects.

The first question asked is whether the CFF scores of any one subject tend to be the same on different days, whilst those of different subjects, tested under comparable conditions, are consistently different. Is the variance "between individuals" significantly greater than the variance "within individuals"? If so, we can assume that individuals differ in CFF, but retain their characteristic fusion frequency—at least over several days. It is, however, not sufficient to divide the total variance into these two components only, because the possibility of some systematic variation over a series of repeated flicker threshold measurements must be taken into account, and the corresponding variance has to be removed from the "within individuals" variance. The necessary subdivisions of the total sum of squares were carried out and summarized in table II, separately for the three illumination groups, and in table III, for all three groups combined. From both tables it can be seen that the variance "between subjects" (rows (1), (6), (11) of table II; third row of table III) is in each case much greater than the corresponding error term (marked by an arrow in the tables). When the  $s^2$  "between subjects" was divided by the  $s^2$  "interaction: subjects  $\times$  days" (Edwards, 1950, pp. 212–19, 291–95), values of  $F$  were obtained which were highly significant in our analysis, for the separate as well as for the combined groups. The assumption that the variances "between individuals" could be obtained by random sampling from a common population variance must be rejected. However, from table I, column "S.D." on the right, it appears that quite considerable variations occur among the scores of some individuals over the 5 days, and one may wonder whether it is the differences in these variations or the individuals' average fusion frequencies which cause the significance of the  $F$  ratios. Bartlett's test for homogeneity of variance (Edwards, 1950, p. 196/7) was applied

TABLE II

(a) Group I (illuminated room):

Source of VARIANCE	Sum of Squares	df	Mean Square $s^2$	F	P <sub>(F)</sub>
between subjects (=row means) (1)	79.204	7	11.315	5.508	<.0005
between days (=column means) (2)	8.004	4	2.001	.974	>.30
interaction: subjects $\times$ days (3)	<u>57.517</u>	<u>28</u>	2.054	$\leftarrow$	
within subjects = (2) + (3) = (4)	65.521	32	2.048		
Total (5)	144.725	39			

(b) Group II (dark room):

Source of VARIANCE	Sum of Squares	df	Mean Square $s^2$	F	P <sub>(F)</sub>
between subjects (=row means) (6)	439.801	7	62.829	21.712	<.0005
between days (=column means) (7)	8.662	4	2.165	.748	>.50
interaction: subjects $\times$ days (8)	<u>81.023</u>	<u>28</u>	2.894	$\leftarrow$	
within subjects = (7) + (8) = (9)	89.685	32	2.803		
Total (10)	529.486	39			

(c) Group III (dimly-lit room):

Source of VARIANCE	Sum of Squares	df	Mean Square $s^2$	F	P <sub>(F)</sub>
(between subjects (=row means) (11)	245.736	7	35.105	16.302	<.0005
between days (=column means) (12)	19.101	4	4.775	2.218	>.05
interaction: subjects $\times$ days (13)	<u>60.294</u>	<u>28</u>	2.153	$\leftarrow$	
within subjects = (12) + (13) = (14)	79.395	32	2.481		
Total (15)	325.131	39			



TABLE III

*Groups I, II and III combined:*

Source of VARIANCE	Sum of Squares	df	Mean Square $s^2$	F	P <sub>(F)</sub>
between groups I, II, III	383.375	2	191.688	5.264	<.025
between subjects in same group, = (1) + (6) + (11)*	<u>764.741</u>	<u>21</u>	36.416 ←		
total, between sub- jects within groups	1148.116	23	49.918	21.088	<.0005
between days (2)* 1, 2, 3, 4, 5 = { + (7)	1.295	4	.324	.137	>.50
interaction: { + (12)	34.472	8	4.309	1.818	>.05
days × groups					
interaction: subjects × days = (3) + (8) + (13)*	<u>198.834</u>	<u>84</u>	2.367 ←		
total, within subjects of 3 groups = (4) + (9) + (14)*	234.601	96	2.444		
Total	1382.717	119			

*Table III.* Summary of analysis of variance "between groups" (I, II, III, tested under different illumination conditions), "between individuals" (i.e. 24 subjects on five days, within their groups), and "between days" (i.e. between the means of 24 subjects on 5 consecutive days).

The arrow ← indicates the mean square ( $s^2$ ) by which the critical one is to be divided for obtaining "F".

\* the numbers in brackets refer to the corresponding rows of table II, a, b, c, which are combined in table III.

The sum of the group totals in table II, rows (5) + (10) + (15) = 999.342; and this sum with the sum of squares "between groups" (= 383.375) adds up to the Grand Total of table III, which may serve as a final check.

*Table II, a, b, c.* Summary of analysis of variance "between individuals" (i.e. the 8 subjects of each group on five days), "within individuals" (the same individuals on five consecutive days), and "between days" (i.e. between the means of 8 subjects on 5 different days), separately for three different conditions of illumination of the test room.

The arrow ← indicates the mean square ( $s^2$ ) by which the critical one is to be divided for obtaining "F".

to the three groups of subjects and the resulting  $\chi^2$  values, based on 7 degrees of freedom, were found to be:

group I  $\chi^2 = 7.08$ ,  $P_{\chi^2} > .30$ , not significant;  
group II  $\chi^2 = 26.45$ ,  $P_{\chi^2} < .0005$ , highly significant;  
group III  $\chi^2 = 10.32$ ,  $P_{\chi^2} > .10$ , not significant.

The variances "between subjects" of groups I and III may be regarded as homogeneous, whilst that of group II is definitely not homogeneous. If we scrutinize table I, b, we see that the CFF (in brackets) of subject 3 on the first day differs quite considerably from her other scores. This fact was explained by the subject as a shift in her standard: on the first day, she judged as "flicker" any slight wavering of the stimulus brightness (which sometimes occurs due to fluctuations of voltage, etc.), but from the second day onward, she responded, more correctly, only to the real rhythmic flicker. It seems justified, therefore, either to remove this subject's scores before testing for homogeneity of variances in group II, or else to substitute a more appropriate score for the questionable one. Omitting her scores, the  $\chi^2$  for the group II variances = 6.78, with a probability  $P_{\chi^2} > .30$ . When the questionable CFF was substituted by the subject's mean CFF (over 5 days, including the high value of the first day),  $\chi^2$  for this group would become 7.09, its probability  $P_{\chi^2} > .30$ . Both these  $\chi^2$  values are in the same order as those of groups I and III.

It may thus be concluded that apart from this inconsistency in the flicker scores of subject 3 of group II, there is no reason for the assumption that heterogeneity of the variances of different individuals are responsible for the significance of the F ratios. We can then assume that it is the individuals' mean fusion frequencies which vary, and must therefore be regarded as typically different for the different subjects of this experiment.

## (2) *Reliability of Flicker Scores*

Having seen that the CFF varies characteristically from individual to individual and that repeated CFF determinations for the same subject are correlated, one may ask how reliable are single measurements of CFF, or, how many CFF determinations are necessary to predict a person's average or true CFF. So far only a tentative answer can be given to these questions.

There are a number of ways of estimating the reliability of the five independent measurements available for each subject. One of these, similar to the split-half reliability coefficient, is the correlation of the average

CFF's of the first, third and fifth days with those of the second and fourth. These correlations, separate for each group and corrected for small sample, are:

group I (illuminated room)	$r = .73$
group II (dark room)	$r = .81$
group III (dimly-lit room)	$r = .78$

As another estimate of reliability, the correlations between the first two and the last three days may be used, which are lower than the odd/even ones, particularly for group I:

group I (illuminated room)	$r = .48$
group II (dark room)	$r = .77$
group III (dimly-lit room)	$r = .71$

For  $n = 8$ , i.e. 6 degrees of freedom, a correlation coefficient of  $r = .707$  is significant at the 5 % level. Although these correlations are based on averages of two and three flicker determinations, they are not very high, being particularly low for group I. Thus it seems that at least two measurements should be made, in order to obtain an approximation of an individual's characteristic CFF.

For two small groups of five subjects each, it was possible to correlate the average CFF (5 days) with the mean of two new flicker determinations after an interval of  $2\frac{1}{2}$  months. The correlation coefficients in this case were:

illuminated test room	$r = .50$
dark test room	$r = .70$

Although these coefficients are not statistically significant for such small numbers of subjects, they correspond with the above-mentioned ones.

In general, these correlation coefficients are in the vicinity of those obtained by other investigators (Misiak, 1948; McNemar, 1951). It can, therefore, be expected that the reliability of a flicker test under optimal measuring conditions is probably about .7 or .8.

### (3) *Practice Effects*

Inspection of the data of table I shows that the average CFF of 8 subjects changes very little over the five days. Only with group III is a consistent increase in the means of the 8 subjects observed. Our variance analysis provides a check on the significance of the changes over the five days of testing. To obtain the critical F ratio, the variance "between days" (rows (2), (7), (12) of table II) must be divided by the corresponding error term,

in this case the variance "interaction : subjects  $\times$  days" (rows (3), (8), (13) of table II), i.e. that portion of the variance "within subjects" which, due to random fluctuations, is not common to all subjects. The only F which is greater than unity is that of group III, but this ratio too has a probability greater than 5 %, and is therefore not significant.

If the three groups are combined, it becomes even more obvious that no systematic influence is effective in the repetition of flicker determinations with the same subjects. The variance „between days" (4th row of table III) is much smaller than the corresponding error term (6th row, table III). It can also be seen that the particular change in the responses of the subjects tested in the dimly-lit room (group III) must not be regarded as significantly different from the more random variations of the two other groups. This is borne out by the ratio of the interaction variances "days  $\times$  groups"/"subjects  $\times$  days" (5th./6th. row of table III) which is not significant.

#### (4) *Effects of Different Illumination Conditions*

The variation of the test room illumination can be expected to influence flicker fusion frequency in at least two ways: (1) it has an effect on the brightness of the stimulus field, and thus on the contrast between stimulus and surrounding; (2) certain changes in the function of the eye, specially in the retina, will occur, if the extreme condition of darkness is included. It is not possible to decide, by the results of our experiment, which of these variables is the essential cause of the observed differences between the three subgroups tested under different levels of room illumination. To answer such questions, more specific variation of the conditions would be necessary. Here, we are only interested in the practical question of whether one or other illumination condition enables us to differentiate more clearly between the typical CFF's of different individuals. And regarding this, the experiment provides some valuable information.

First of all, the variance analysis has to prove whether the observed differences between the three illumination groups exceed chance differences. Since the ratio of the variances "between groups"/"within groups (= between subjects)" is 5.264 and is significant at the 5 % point (table III, first two rows), this is definitely so. Originally, the within groups variance was further analysed into the subcomponents "between subjects" and "interaction : subject  $\times$  groups". Since, however, these variances were practically the same (37.765 with 7 df, 35.742 with 14 df), they could be combined and used for testing the ratio "between/within groups" with 2 and 21 df, respectively, as is done in table III.

Knowing that the differences between groups I, II and III are significant, we may examine their meaning in view of our practical question. If we inspect the standard deviations of the CFF scores listed in table I (values in brackets, at the right hand and bottom margins), we find that there is a consistent shift in the relative size of the S.D. within individuals as compared with that between individuals, the intra-individual S.D. decreasing relatively from the illuminated over the dimly-lit to the dark test room. The same fact is reflected, reciprocally, by the increase in the F ratios (between subjects/interaction: subjects  $\times$  days) from group I over group III to group II (table II, rows (1), (11), (6)). In order to exclude the possibility that the standard deviations are influenced by the means of CFF—e.g. exaggerated when taken as deviations from a higher mean CFF, where it may be more difficult to distinguish between two adjacent frequencies—variability coefficients,  $V = \frac{100 \text{ S.D.}}{\text{Mean}}$ , and the ratios of *intraindividual* to *interindividual* variability (based on the „within”/“between subjects” variances) were computed. These ratios were found to be:

$$\begin{aligned} \frac{\text{intra-individual variab.}}{\text{inter-individual variab.}} &= .2112 \text{ dark room} \\ &= .2658 \text{ dimly-lit room} \\ &= .4253 \text{ illuminated room} \end{aligned}$$

In other words, the intraindividual fluctuations in a series of repeated measurements—equivalent to a proportion of unreliability of the test—were about 21 %, 27 % and 43 %, respectively, of the characteristic variability in fusion frequency between different subjects. When this proportion is small, the measurement of CFF is reliable, and vice versa. The best condition of the three tried out in the present experiment, seems to be that of the completely dark test room, and the next best that of the dimly-lit room. As it is easier to control the state of the subject's dark adaptation in a dimly-lit room, and since the subject may feel more comfortable when there is some light, the condition most suitable as a standard test situation for measuring individual differences in flicker fusion frequency is the dimly-lit one.

##### (5) *Correlation between Alpha Frequency of EEG and Flicker Fusion Frequency*

If the method of determining an individual's flicker threshold, as described in the previous paragraphs, is correct and yields genuine and reasonably reliable measures of a certain parameter, what then is the value

of these measurements? This is, of course, a major problem, and we do not claim to be able to give a complete answer to it.

Our basic hypothesis is that CFF depends on some fundamental neurophysiological processes which are also partly responsible for differences in temperamental qualities. To relate CFF directly to certain subjective temperament assessments is not advisable, because it would be necessary to work with very large samples which are as yet not available. And, as the techniques of subjective temperament assessment are still in need of improvement, low correlations might be obtained, which would be difficult to interpret.

CFF "has certainly been demonstrated to be a sensitive indicator of neurological efficiency" (Landis, 1951, p. 314). We should therefore look for a criterion which is also a neurological measure, preferably one of high reliability, known to be related to temperament. The only measure which meets these requirements is frequency of alpha rhythm in the EEG. According to Biesheuvel and Pitt (1955) and Mundy-Castle (1955), the frequency of the alpha rhythm is negatively correlated with the temperament variable Secondary Function (or positively correlated with Primary Function). Mundy-Castle has also given a convincing explanation of the relationship between rapidity of discriminative perception and alpha frequency. From these findings and from the generally held supposition, that Secondary Function is connected with a certain inertia in the processes of the central nervous system, it follows that CFF and alpha frequency should be positively correlated.

For 23 of the subjects who participated in the present flicker experiments, EEG reports (recorded during the previous two years) were available. To correlate these with the subjects' CFF values, the latter had first to be made comparable. That is the CFF values obtained under varying conditions had to be transformed into scores corresponding to only one condition of illumination. This was achieved by repeating flicker determinations for a number of subjects, but now under one of the *other* conditions. The average quotients of the subjects' two mean CFF's were then used as transformation factors. All CFF scores determined in the illuminated or dark room were converted into those corresponding to the conditions of the dimly-lit room. The inaccuracy that might have been introduced by this transformation may be neglected for the time being, since it is not likely to influence the correlation with any other variable systematically.

The correlation between CFF and alpha frequency was found to be  $r = +.545$ , which, for  $n = 23$ , is significant at the 1 % level. This result, based on 23 cases only, must, of course, be confirmed by further experi-

ments along the same or similar lines. It does, however, indicate that the individual flicker threshold really seems to depend on some neurophysiological factor or factors which are related to differences in alpha frequency and operative in one at least of the basic temperament variables.

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#### SUMMARY

Some limitations of the known method of determining individual flicker threshold are considered, and an apparatus is described which eliminates the disturbing systematic influences of practice and expectation.

This apparatus consists of the combination of a small projector and an episcope. It can produce different flicker frequencies by the use of a set of discs, each of which has a different number of holes drilled round a circumference. Thus it is possible to present to the subject single flicker stimuli in a random order, which are completely free from any acoustic or extraneous visual clues. As the apparatus is operated from a cubicle, the stimulus projected into the testing room can be observed under different conditions of visual adaptation and room illumination, including darkness, without hampering the experimenters' manipulations and recording.

Results of the main experiments with 3 groups, each of 8 subjects, under 3 different conditions of test room illumination are as follows:

- (1) Variations of CFF "between individuals" were significantly greater than those "within individuals" (over 5 days). As this is due to differences in the individuals' average fusion frequencies and not to heterogeneity of variances, CFF must be regarded as a personal characteristic.
- (2) Test-retest reliability was tentatively determined as .7, for the most suitable testing conditions. Because of this relatively low reliability, individual CFF's should be measured at least twice.
- (3) No systematic variations of CFF were found when measured with the present apparatus over 5 consecutive working days.
- (4) For three illumination conditions, illuminated, dark and dimly-lit test room, significant differences in the variances of the three groups of subjects were obtained. The ratio intra-/inter-individual variability was smallest for the dark test room and greatest for the illuminated one, indicating that in this experiment the dark room produced the most reliable CFF scores.
- (5) A significant correlation of  $r = +.545$  ( $n = 23$ ) was found between CFF and alpha frequency of EEG. CFF seems to measure a function of the same central nervous processes to which differences both in alpha rhythm and in the temperament variable Primary-Secondary Function may be attributed.



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CONTRIBUTION TO THE STUDY OF THE SIZE-WEIGHT  
ILLUSION BY THE METHOD OF P. KOSELEFF

BY

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The phenomenon of the size-weight illusion is well known by the psychologists; consequently, it appears superfluous to us here to make a description of it. Let us simply remind that, when one lifts two objects of the same weight but of different sizes, the smaller is perceived as the heavier.

Whichever their technique may have been, the majority of the authors have found a great constancy of the illusion, as much with normal subjects as with abnormal ones. Claparède observed the existence of the illusion in 93 % of the pedagogical backward subjects of the special classes in Geneva. A. Doll, on his side, detected the illusion in 100 % of the abnormal subjects whose mental age reached 8 years.

In order to study the illusion referred to, we have used, in a previous work, as weights which had to be compared by lifting, parallelepipeds in beechwood. One of them was manifestly less high than the 13 others; it was 21 cm long, 5 cm broad and only 2 cm high. The 13 large blocks had the same basis as the little one, but they were 7 cm high. The little block and the lightest of the series of the 13 large blocks, both weighed 500 gr. The 12 other large blocks weighed respectively: 550, 600, 650, 700, 750, 800, 850, 900, 950, 1000, 1050 and 1100 gr.

When we have each of the large blocks compared with the little one, according to a technique and processes, which we have described in two previous studies, we have observed that:

- (a) in the 600 adult and normal subjects we have examined, the illusion could be traced in 99,17 % of the cases, when the comparison was made between the large and the little block, both weighing 500 gr;
- (b) in 260 adult and normal subjects, the magnitude of the illusion resulting from the comparisons of each one of the blocks of a higher weight with the little block, was generally very important.

By applying of the same technique upon pathological subjects, constituting

a group of 68 general paretics, another of 42 senile patients and a third one of 97 adult oligophrenics, all generally very deficient, we have observed:

- (a) the 110 dement subjects all showed the illusion, which means that with them the frequency of the illusion is of the same rate as with normal people (99,17 %); with the feeble-minded subjects, this frequency was reduced to 94,8 %;
- (b) in all the dement subjects examined and in the 94,8 % of the feeble-minded ones, who showed the illusion, the magnitude of the latter was of the same rate as in normal adults.

From these observations, we may infer that:

- (1) the ordinary proof of the size-weight illusion, even when it is much differentiated, has no clinical value at all with the intellectually deteriorated patients;
- (2) this proof, even when much differentiated, has hardly any clinical value toward the examination of adult oligophrenics.

We have then wondered whether the method of P. Koseleff could not constitute, owing to a greater sensibility, a proof of a certain clinical value. This author, in order to study analytically the size-weight illusion in normal man has used two parallelipipeds (the instrumentation from which we have derived our above mentioned scale): the one is flat and is 21 cm long, 5 cm

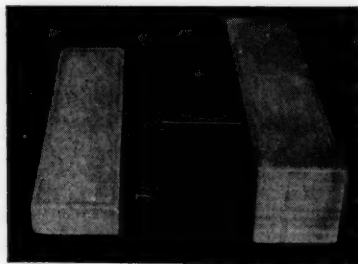


Fig. I. P. Koseleff's instrumentation

broad and 2 cm high; the other has the same basis but is 5 cm high. The little block is much heavier than the large one. Koseleff asks the subject first to lift the smaller block, then to place it upon the larger in order to lift them together. The subject is asked to say what becomes of the weight when both blocks are taken together. The overgreat majority of the subjects answer that both blocks joined are not so heavy as the little block lifted alone.

The method which we applied previously in order to establish the magnitude of the illusion is somewhat akin to Koseleff's, as we asked the subject to compare our little block with the larger ones. Now, our little volume had the same dimensions as the little one used by Koseleff, whereas our larger volumes had the same dimensions as the two blocks of Koseleff

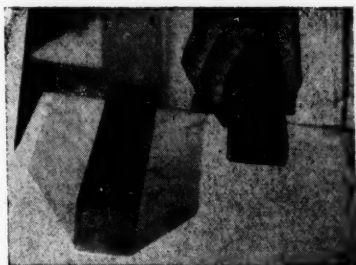


Fig. II. Lifting of the small block (b)



Fig. III. Lifting of the superposed blocks ( $b + B$ )

placed on top of each other. In fact, in the method such as Koseleff has applied it, the subject has to compare the weight of a little block with the one of two blocks which, being joined, have inevitably a higher physical weight. It is certain that in Koseleff's own method a supplementary mental

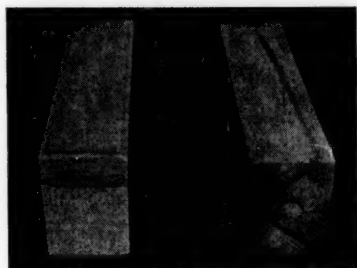


Fig. IV. The superposed blocks of Koseleff's method and one of the large blocks used in our previous experiments

factor intervenes, whose importance may not be neglected: the subject may be aware of the fact that, through the superposition of the two blocks, the actual weight must increase; but when lifting simultaneously both blocks he clearly gets the impression that the weight is diminishing, which, according to several subjects, constitutes a paradoxical situation. We have thought that this factor "paradox" and the fact that a little lighter weight has to be compared with a heavier large volume could produce a greater sensibility of the proof, i.e. a less great frequency of the illusion in the normal and the pathological subjects.

Our researches were made with 5 different groups: two groups of normal subjects, one group of senile patients, another one of general paretics and a last one constituted by adult oligophrenics. For all these groups, the experiment was a double one:

- 1° to lift the little block, place it on top of the larger one and, without interruption of the act, lift both volumes together;
- 2° to lift first both blocks together, and then, again without interrupting the movement, lift the little volume alone.

After each of these proofs, the subject had to give his impression about the eventual variation of the weight and about the sense of this variation.

*1st. Group:* 100 normal adult subjects.

The little volume has a weight of 700 gr, the larger weighs 100 gr.

*Results:*

- process no. 1. in 92 % of the cases, both blocks together seem lighter than the little one lifted alone;  
in 5 % of the cases, the subject does not experience any change of weight;  
3 % of the subjects declare that the blocs taken together are heavier.
- process no. 2: for 97 % of the subjects, the blocks taken together are lighter than the little block lifted alone;  
3 % of the subjects declare that they do not perceive any change of weight when the volumes are being separated;  
no subject found the little block lighter than both taken together.

*2nd. Group:* 200 normal adult subjects.

In order to give more sensibility to the proof, we have replaced the block of 700 gr by a similar volume weighing only 500 gr. A greater

sensibility must be given indeed, through the reduction of the difference between the weight of the little volume and that of the larger one.<sup>1</sup>

### Results:

- process no. 1: in 90 % of the cases, both blocks taken together seem lighter than the little one lifted alone;  
 in 5 % of the cases, the subjects do not find any change;  
 5 % of the subjects declare that the blocks taken together are heavier.
- process no. 2: for 94,5 % of the subjects, the blocks taken together are lighter than the little block alone;  
 4,5 % of the subjects declare that they perceive any change in weight when they separate the volumes;  
 in 1 % of the cases, the little block weighs less than both blocks taken together.

TABLE I

Blocks	Processes	$B + b < b$	$B + b = b$	$B + b > b$	Illusion
b 700 and B 100	I	92 %	5 %	3 %	97 %
	II	97 %	3 %	0 %	100 %
b 500 and B 100	I	90 %	5 %	5 %	95 %
	II	94.5 %	4.5 %	1 %	99 %

In reality, as in both processes, 1 and 2, it comes to comparative weighings between a volume  $b$  (700 or 500 gr) and a volume  $B + b$  (800 or 600 gr), the illusion exists not only when the subject finds  $B + b$  lighter than  $b$ , but also when he thinks that  $B + b = b$ .

As the experiments for which we have used a block of 700 gr have proved less sensible than the ones in which  $b$  weighed 500 gr, we shall compare only the results of our latter experiments with those of the method we have mentioned at the beginning of the present study.

The frequency of the illusion when comparing two different sizes, which are of equal weight (500 gr), was of the rate of 99,17 %, whereas Koseleff's method gives an illusion-frequency of 95 % (process 1), and of 99 % (process 2).

<sup>1</sup> In order to eliminate the eventual influence of the order of succession of the two groups upon our results, we have applied process no. 1 first, in 100 cases, and process no. 2 first, in 100 other cases.

It is however more indicated to compare the results obtained through application of Koseleff's method with the ones supplied by our previous experiments, when we compared the little block of 500 gr with the large one of 600 gr. In Koseleff's method, indeed, the subject had to compare a little block of 500 gr with two superposed blocks which, together, weighed 600 gr and had the same dimensions as those of the large blocks used in our previous work.

This comparison teaches us that the illusion-frequency, obtained through our previous method, and the one supplied through Koseleff's method, are respectively of 98 % against 95 (process 1) and 99 % (process 2). Consequently, it seems that we are not allowed to attribute a greater sensibility to Koseleff's proof in its practical application.

*3rd. Group:* 50 subjects suffering from paretic dementia.

Only the blocks b 500 gr and B 100 gr have been used.

With process 1, the illusion was present in 94 % of the cases; with process 2, in 96 %.

*4th. Group:* 20 subjects suffering from senile dementia.

Blocks used: b 500 gr, B 100 gr.

Process 1 gives an illusion-frequency of 85 %, and process 2 in 90 % of the cases.

*5th. Group:* 33 adult oligophrenic subjects.

Blocks used: b 500 gr and B 100 gr.

Process 1: presence of the illusion in 94 % of the cases; process 2: presence of the illusion in 100 % of the cases.

TABLE II

Subjects	Processes	B + b < b	B + b = b	B + b > b	Illusion
50 general paretics	I	90 %	4 %	6 %	94 %
	II	94 %	2 %	4 %	96 %
20 senile patients	I	85 %	0 %	15 %	85 %
	II	90 %	0 %	10 %	90 %
33 oligophrenic adults	I	94 %	0 %	6 %	94 %
	II	100 %	0 %	0 %	100 %

The frequency of the illusion with the 110 demented examined with our previous method (comparison of b 500 gr with B 600 gr) was of 95,6 % in the 68 general paresics, and of 95,2 % in the 42 senile patients. In the 97 adult oligophrenics, examined in the same way, the frequency of the illusion was of 88,7 %.

In pathological cases, we observe consequently that the differences of the results obtained by Koseleff's method, on the one hand, and those supplied by a more current method, on the other hand, are not sufficiently distinct to be retained in favour of a greater sensibility of Koseleff's method—at least in a practical realm as e.g. the clinic.

We observe, moreover, that the frequency of the illusion obtained through Koseleff's method with the cases of even severe mental deficiency or deficit, differs hardly at all from the one found in normal subjects. This confirms our previous observations through the application of a simpler method, i.e. that the frequency of the illusion is of the same rate in normal adults as in deficient and deteriorated ones.

To end this study, we should like to draw the attention of the reader upon two facts, which become evident from our previous studies and equally from the present researches:

- (1) The adult oligophrenics show an illusion-frequency only slightly different from the one observed in normal adults. This observation is in agreement with Claparède's, made upon pedagogically backward pupils (*arriérés pédagogiques*), and with Doll's, made upon feeble-minded subjects, whose level reached a mental age of 8 years. These data might seem in contradiction with those of some authors, amongst whom we should quote especially J. Demoor, who have reported the absence or the inversion of the size-weight in feeble-minded children. We are of opinion that this contradiction is only apparent, for, according to the data of Claparède, Doll and ourselves, we have every reason to think that an insufficiency or a retardation of the mental development do not hinder a (possibly slower) acquirement of the illusion, under the influence of the practical experience of everyday life.
- (2) The size-weight illusion, once it is acquired, proves to be very tenacious, to such an extent that it does not vanish in the least in cases of even considerable mental deterioration.

## SUMMARY

By our previous researches, it was observed that the size-weight-illusion was very constant in normal adult subjects. The constancy of this illusion was equally great in the senile patients and general paretics, and, in a slightly lower measure, in oligophrenic adults.

Koseleff's experiment, which consists in having the subject compare a weight of little size with the same weight to which is added a large, but light, volume, gave us the idea that this method might constitute a more sensible proof and, consequently, produce a less tenacious illusion, particularly in pathological cases with pronounced intellectual deficiency or deterioration.

We have applied Koseleff's method upon 300 normal adult subjects, 50 general paretics, 20 senile patients and 33 adult oligophrenics.

Through application of the method mentioned, the illusion has appeared to be as frequent as through the application of a more current method, with normal people as well as with pathological subjects.

Koseleff's method, consequently, which is so very interesting from other points of view, proves, to the same extent as a more current method for the examination of the size-weight-illusion, clinically worthless for the detection of mental deficiency or deterioration in adults.

## RÉSUMÉ

Par nos recherches antérieures, il a été constaté que l'illusion poids-volume était très constante chez les sujets adultes normaux. La constance de cette illusion était tout aussi grande chez les déments séniles, les paralytiques généraux, et dans une mesure légèrement moindre, chez les oligophrènes adultes.

L'expérience de Koseleff, qui consiste à faire comparer un poids de petit volume avec ce même poids additionné d'un grand volume léger, nous a donné l'idée que cette méthode pourrait peut-être constituer une épreuve plus sensible et par suite donner lieu à une illusion moins tenace, en particulier chez les cas pathologiques avec déficit intellectuel prononcé.

La méthode de Koseleff a été appliquée chez 300 sujets normaux adultes, chez 50 paralytiques généraux, chez 20 déments séniles, et chez 33 oligophrènes adultes.

Par l'application de la méthode en question, l'illusion s'est montrée pratiquement aussi fréquente que dans l'application d'une méthode plus courante, tant chez les normaux que chez les pathologiques.

En conséquence, la méthode de Koseleff, par ailleurs si intéressante, se montre, tout comme une méthode plus courante d'examen de l'illusion poids-volume, sans valeur clinique pour le dépistage de la déficience ou de la détérioration de l'intelligence chez l'adulte.



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## PHÄNOMENALE GESCHWINDIGKEITSUNTERSCHIEDE BEI BEWEGUNGEN IN VERSCHIEDENEN RICHTUNGEN

VON

H. C. VAN DER MEER

### 1. FRAGESTELLUNG

J. F. Brown beschrieb in seinen Aufsätzen „Über gesehene Geschwindigkeiten“ (Ps. Forschung 10, 1927) und „The visual perception of velocity“ (Ps. Forschung 14, 1931) die Faktoren, welche die phänomenale Geschwindigkeit sich bewegender Objekte beeinflussen. In einem dieser Versuche erwähnt er einen Unterschied zwischen einer horizontalen und einer vertikalen Bewegung, und zwar ist bei gleicher objektiver Geschwindigkeit die vertikale Bewegung phänomenal schneller als die horizontale ( $V_h/V_v = 1.30$ ). Die phänomenale Geschwindigkeit einer Bewegung, die unter einem Winkel von  $45^\circ$  mit der Horizontalen und der Vertikalen verläuft, liegt in der Mitte beider ( $V_d/V_v = 1.10$ ).

Dies führt zu der Frage nach dem Einfluss der Orientierung auf die phänomenale Geschwindigkeit. M.a.W. ist eine objektiv vertikale Bewegung in dem Sinne, dass dieser phänomenale Geschwindigkeitsunterschied beibehalten bleibt, immer vertikal unabhängig von der Haltung des Kopfes oder der Stellung des Körpers der Versuchsperson bezüglich der Gravitation?

Die zweite Frage, mit der wir uns beschäftigen wollen, ist: gibt es mehrere Asymmetrien in der phänomenalen Geschwindigkeit der Bewegungen, die in den Hauptrichtungen unseres visuellen Wahrnehmungsraumes verlaufen? D.h. gibt es bei gleicher objektiver Geschwindigkeit phänomenale Geschwindigkeitsunterschiede einerseits zwischen einer nach rechts und einer nach links gerichteten horizontalen Bewegung, andererseits zwischen einer nach oben und einer nach unten gerichteten vertikalen Bewegung?

Die beiden Fragen wollen wir in den nachstehenden Paragraphen behandeln, zuerst aber die befolgte Methode und die Apparatur beschreiben.

### 2. METHODE

Die Versuchspersonen erhielten den Auftrag, die Geschwindigkeit von zwei sich bewegenden Objekten miteinander zu vergleichen. Es war dabei nicht gestattet, Hilfsmittel wie z.B. Zählen u.ä. zu benutzen, sondern die Versuchspersonen sollten nur den unmittelbaren Eindruck, den die Schnelligkeit auf sie machte, beobachten.

Die sich bewegenden Objekte waren zwei Lichtpunkte mit einem Durchmesser von 14 mm, die auf einen Schirm in einer frontal-parallelen Fläche nacheinander projiziert wurden (nur in den ersten orientierenden Versuchen war der Durchmesser grösser). Dieser Schirm war aus einem Bogen schwarzer Pappe von  $55 \times 65$  cm hergestellt, in dessen Mitte ein Viereck von  $15 \text{ cm}^2$  ausgeschnitten war, auf das Schreibmaschinenpapier geklebt wurde, so dass die Lichtpunkte auf der Hinterseite deutlich wahrnehmbar waren. Die Versuchspersonen sassen hinter dem Schirm in einer Entfernung von 1.20 m. Der Schirmausschnitt von  $15 \times 15$  cm, auf dem sich die Lichtpunkte bewegten, befand sich ungefähr in Augenhöhe. Die Bewegungen der Lichtpunkte kreuzten sich in der Mitte des Feldes, so dass der Schnittpunkt der horizontalen und der vertikalen Bewegung mit demjenigen der Diagonalen des Schirmausschnittes zusammenfiel.

Die Lichtpunktquelle war eine Glühbirne von 0.45 A, 8 V, die sich in einer kleinen Hülse befand und vor welcher eine positive Linse angebracht war. Das austretende Strahlenbündel fiel auf einen Spiegel, vor dessen unmittelbarer Nähe noch eine zweite positive Linse aufgestellt war. Dieser Spiegel war an einer Scheibe befestigt, welche durch einen Elektromotor in Bewegung gesetzt werden konnte, dessen Geschwindigkeit variabel war. Das Lichtbündel der Glühbirne wurde von dem rotierenden Spiegel als ein sich bewegendes Lichtpunkt auf den Schirm projiziert. Der Abstand vom Schirm bis zu den Spiegeln betrug ca. 1.65 m.

Die Grösse der Lichtpunkte konnte durch ein Diaphragma vor der ersten Linse beliebig geändert werden; die Lichtintensität der beiden Punkte war durch Einschaltung eines Widerstandes zu regeln. Die Abstände von der ersten Linse bis zur Glühbirne und von der zweiten Linse bis zum Spiegel, sowie der Abstand der Glühbirne bis zum Spiegel und die Stärke der Linsen sollten so bestimmt werden, dass die Lichtpunkte auf dem Schirm völlig scharf und identisch waren.

Die Geschwindigkeit der horizontalen Bewegung wurde konstant gehalten, die der vertikalen Bewegung war variabel. Die Bewegungen wurden sofort nacheinander gezeigt.

Die Versuche wurden im Dunkeln vorgenommen. Da aber die Lichtbündel der beiden Glühbirnen ein sehr schwaches Licht verbreiteten, war es nicht vollkommen finster, sondern konnten die Versuchspersonen — nachdem sie sich an die Dunkelheit gewöhnt hatten — die Umrisse der Objekte im Zimmer sehr verschwommen wahrnehmen. Wahrscheinlich entspricht dies ungefähr den Bedingungen Browns, der seine Versuche auch nicht in völliger Finsternis angestellt hat.

Die Versuche wurden in solcher Weise vorgenommen, dass die Versuchsperson, die zwei nacheinander erscheinenden Geschwindigkeiten miteinander zu vergleichen und solange der Vp. Anweisungen für Veränderung der zweiten Geschwindigkeit zu geben hatte, bis ihr beide gleich erschienen. Für jede Vp. wurden 6 bis 8 Einstellungen gemacht, deren Durchschnittswerte in den nachstehenden Tabellen angeführt sind. Es wurde dabei abwechselnd angefangen mit einer Geschwindigkeit, die entweder bedeutend grösser oder bedeutend kleiner war als die Standardgeschwindigkeit. Dadurch hatte die Vp. zunächst den Eindruck eines deutlichen Geschwindigkeitsunterschiedes, worauf dann die Annäherung an Gleichheit erfolgen konnte. Wenn die Vp. meinte, dass die Geschwindigkeiten der beiden Punkte übereinstimmten, wurde die Geschwindigkeit der Punkte festgestellt, indem die Zeit, die der Punkt horizontal über einen Abstand von 80 cm, vertikal über einen Abstand von 60 cm durchlief, mit einer Stoppuhr gemessen wurde.

Obgleich die Versuche im Dunkeln erfolgten, und die VI. also die Geschwindigkeit auf der Skala-einteilung des Motors nicht ablesen konnte, wurde bei jeder Vp. kontrolliert, ob von der VI. eine suggestive Wirkung ausgehen könne. Zu diesem Zwecke veränderte die VI., wenn der Geschwindigkeitsunterschied für die Vp. nur noch sehr gering war, einige Male die Geschwindigkeit der Variablen, gegensätzlich der Anweisung der Vp. In keinem einzigen Falle konnte die VI. auf diese Weise das Urteil der Vp. beeinflussen.

Da bei diesen Untersuchungen vorausgesetzt wird, dass man dazu fähig ist, zwei gesehene Geschwindigkeiten einigermaßen genau miteinander zu vergleichen, wurde dies zuerst kontrolliert. Bei zwei Vpn. wurden auf obenerwähnte Weise 6 Einstellungen gemacht um die subjektive Geschwindigkeitsgleichheit zweier sich in gleicher Richtung bewogender Objekte zu bestimmen. Es stellte sich heraus, dass der Sukzessivvergleich zweier Geschwindigkeiten in gleicher Richtung ganz gut gelingt; für beide Vpn. wich der Unterschied der objektiven Geschwindigkeit des zweiten Punktes bei subjektiver Gleichheit weniger als 1 % von der Standardgeschwindigkeit ab.

Die Vpn. waren zum Teil Studenten, die am Anfang ihres Studiums standen und zum Teil Angestellte des Laboratoriums. Gerne sage ich ihnen meinen Dank für ihre Bereittheit, sich als Vp. zur Verfügung zu stellen.

### 3. VERSUCHE ÜBER HORIZONTALE UND VERTIKALE BEWEGUNG

Brown, der in seinen Versuchen über die phänomenale Geschwindigkeit sich bewogender Objekte mit schwarzen Punkten arbeitete, die sich auf einem beleuchteten Feld bewogen, stellte fest, dass eine Bewegung in vertikaler Richtung phänomenal schneller ist als eine Bewegung in horizontaler Richtung ( $V_h/V_v = 1.30$ ). Die bewogenden Objekte in meinen Versuchen waren dagegen Lichtpunkte, die auf ein weisses Feld projiziert wurden. Zuerst musste also untersucht werden, ob der von Brown entdeckte Unterschied in phänomenaler Geschwindigkeit zwischen einer horizontalen und einer vertikalen Bewegung auch unter diesen Bedingungen auftrat.

Zu diesem Zwecke wurden zunächst einige orientierende Versuche angestellt.

*Versuch 1.* Lichtpunkte mit einem Durchmesser von 40 mm wurden auf einen weissen Schirm von  $46.2 \times 46.2$  cm projiziert. Der Beobachtungsabstand war 1.20 m. Die Geschwindigkeit der Standardbewegung, von rechts nach links laufend,  $V_h$ , war 5.5 cm/sec. Die vertikale Bewegung lief von unten nach oben. Die Wahrnehmung war binokular, ohne Fixation.

TABELLE 1  
Feldgrösse  $46.2 \times 46.2$  cm,  $V_h$ : 5.5 cm/sec.

Beobachtung binokular			Beobachtung monokular		
Vpn.	$V_h/V_v$	$\sigma$	Vpn.	$V_h/V_v$	$\sigma$
vdM	1.30	1.02	vdM	1.24	0.81
S	1.12	1.56	S	1.11	0.97

*Versuch 2.* Wie Versuch 1; die Wahrnehmung jedoch monokular, ohne Fixation.

$V_h/V_v$  bezeichnet das Verhältnis der objektiven Geschwindigkeiten bei subjektiver Gleichheit. Die Zeitmessungen bilden die Basis für die Berechnung der Streuungen.

*Versuch 3.*  $V_h = 21$  cm/sec.; im übrigen wie Versuch 1.

*Versuch 4.*  $V_h = 21$  cm/sec.; im übrigen wie Versuch 2.

TABELLE 2

Feldgrösse  $46.2 \times 46.2$  cm,  $V_h: 21$  cm/sec.

Beobachtung binokular			Beobachtung monokular		
Vpn.	$V_h/V_v$	$\sigma$	Vpn.	$V_h/V_v$	$\sigma$
vdM	0.80	0.15	vdM	0.81	0.18
S	0.90	0.73	S	0.80	0.20

Vergleichen wir die Resultate aus den Tabellen 1 und 2, so ergibt sich daraus, dass bei einer Geschwindigkeit der Standardbewegung von 5.5 cm/sec., die phänomenale Geschwindigkeit der vertikalen Bewegung die grössere ist; jedoch bei einer Geschwindigkeit von 21 cm/sec. diejenige der horizontalen Bewegung.

Es wurden nun einige Änderungen in der Versuchsanordnung vorgenommen um zu prüfen, ob dieses Resultat persistent sei. Der Schirm bestand jetzt aus schwarzer Pappe von  $55 \times 65$  cm, aus dessen Mitte ein Viereck von 30 cm<sup>2</sup> ausgeschnitten wurde, das mit weissem Schreibmaschinepapier beklebt wurde.

*Versuch 5.* Feldgrösse  $30 \times 30$  cm, Punktdurchmesser 30 mm, Beobachtungsabstand 1.20 m. Wahrnehmung binokular, ohne Fixation. Die Standardbewegung lief von rechts nach links, die Variable von unten nach oben.  $V_h: 5$  cm/sec.

*Versuch 6.* Wie Versuch 5;  $V_h: 10$  cm/sec.

*Versuch 7.* Wie Versuch 5;  $V_h: 20$  cm/sec.

Die Ergebnisse dieser drei Versuche sind in Tabelle 3 zusammengefasst. In diesen und den nachfolgenden Versuchen wurde die Zeit für die horizontale Bewegung über einen Abstand von 80 cm, für die vertikale Bewegung über 60 cm gemessen. Die Zeitmessungen bilden die Basis für die Berechnung der Streuungen.

TABELLE 3

Feldgrösse 30 × 30 cm, Beobachtung binokular, Bewegungsrichtungen  $\leftarrow \uparrow$ 

$V_h: 5 \text{ cm/sec.}$			$V_h: 10 \text{ cm/sec.}$			$V_h: 20 \text{ cm/sec.}$		
Vpn.	$V_h/V_v$	$\sigma$	Vpn.	$V_h/V_v$	$\sigma$	Vpn.	$V_h/V_v$	$\sigma$
vdM	1.10	0.24	vdM	0.98	0.28	vdM	0.90	0.09
S	1.12	1.56	S	0.90	0.45	S	0.95	0.21
Y	1.13	1.50	Bl	1.10	1.02	vG	0.90	0.37
W	0.87	0.14	W	0.90	0.14	W	1.23	0.38
C	1.06	1.66				C	1.50	0.25
F	0.75	1.60				Ba	1.11	0.32
B	0.97	0.37						
Z	0.90	0.62						

Die Ergebnisse in der Tabelle 3 entsprechen jedoch dem Resultat Browns nicht. Der phänomenale Geschwindigkeitsunterschied zwischen der horizontalen und der vertikalen Bewegung ist weniger ausgeprägt. Die Geschwindigkeit, bzw. die Expositionszeit, scheint die Richtung dieses Unterschiedes zu beeinflussen, und ausserdem sind die interindividuellen Unterschiede gross. Muss man nun etwa zwei Typen Vpn. annehmen, einen der bei geringerer Geschwindigkeit die horizontale Bewegung, bei grösserer Geschwindigkeit die vertikale Bewegung überschätzt, während bei dem anderen Typus diese Illusionen gerade umgekehrt sind? Für diese Annahme könnte sprechen, dass nach Sleight und Austin (J. of Psychology 1952) die klassische, statische horizontal-vertikale Illusion in der Reihe Viereck—Rechteck nicht allgemein gültig wäre, doch dass es Personen mit einer klassischen und einer kontraklassischen Illusion, sowie auch solche ohne Illusion, gäbe. Es ist nicht ohne weiteres klar, wie diese Tendenz auf die hier beschriebene Bewegungsillusion von Einfluss sein könnte und warum der Geschwindigkeitsfactor, bzw. die Expositionszeit, eine umkehrende Wirkung auf das Verhältnis der Illusionen ausüben würde.

Da es in erster Linie die Absicht war, zu prüfen, ob unter den hier geltenden Bedingungen eine gleiche Bewegungsillusion als von Brown entdeckt wurde, vorkomme, wollen wir jetzt dieses Problem nicht weiter verfolgen, sondern versuchen, ob wir, indem wir die Momente Geschwindigkeit, Grösse des bewegenden Punktes und Feldgrösse möglichst genau mit den von Brown verwendeten Grössen in Übereinstimmung bringen, eine gleich grosse Illusion ermitteln können, die annähernd für alle Vpn. gültig ist.

Versuch 8. Feldgrösse 15 × 15 cm, Punktdurchmesser 14 mm,

Beobachtungsabstand 1.20 m.  $V_h$ : 10 cm/sec. Die Wahrnehmung war binokular, ohne Fixation.

Die Pfeile in den Tabellen bezeichnen die Richtungen der Bewegungen.

TABELLE 4

Feldgrösse  $15 \times 15$  cm, binokulare Beobachtung.  $V_h$ : 10 cm/sec.  
Bewegungsrichtungen  $\leftarrow \uparrow$

Vpn.	$V_h/V_v$	$\sigma$
vdM	1.03	0.25
W	1.02	0.14
dJ	1.14	0.39
V	1.03	0.44

Versuch 9. Wie Versuch 8, mit dem Unterschied, dass die horizontale Bewegung von links nach rechts verlief.

Versuch 10. Wie Versuch 9, jedoch die vertikale Bewegung von oben nach unten verlaufend.

TABELLE 5

Feldgrösse  $15 \times 15$  cm,  $V_h$ : 10 cm/sec. Beobachtung binokular

Bewegungsrichtungen	$\rightarrow \uparrow$	$\rightarrow \downarrow$
Vpn.	$V_h/V_v$	$V_h/V_v$
vdM	1.31	1.29
W	1.24	1.24
dJ	1.34	1.35
V	1.36	1.36

Aus Tabelle 5 ersehen wir, dass die von Brown entdeckte Bewegungsillusion unter diesen Bedingungen tatsächlich auftritt.

Aus den Versuchen 8 bis 10 ergibt sich demnach, dass man unter den hier beschriebenen Bedingungen *nicht im allgemeinen von einer horizontal-vertikalen Bewegungsillusion reden kann, sondern dass diese Illusion nur zwischen einer nach rechts gerichteten horizontalen und einer vertikalen Bewegung auftritt*. Zwischen einer nach oben und einer nach unten gerichteten Bewegung scheint kein Unterschied zu bestehen.

Im nächsten Paragraphen werde ich auf den hier gefundenen Unterschied zwischen einer nach rechts und einer nach links gerichteten horizontalen Bewegung zurückkommen; dabei werde ich dann zugleich den Einfluss des Zeitfehlers, der in diesen Versuchen nicht berücksichtigt

wurde, in Betracht ziehen. Jetzt wollen wir die weiteren Versuche zwischen der horizontalen und der vertikalen Bewegung verfolgen.

*Versuch 11.* Wie die Versuche 8 und 9; die Wahrnehmung jedoch monokular, ohne Fixation.

TABELLE 6

Feldgröße  $15 \times 15$  cm,  $V_h$ : 10 cm/sec. Beobachtung monokular

Bewegungsrichtungen	$\rightarrow \uparrow$	$\leftarrow \uparrow$
Vpn.	$V_h/V_v$	$V_h/V_v$
vdM	1.38	1.08
S	1.23	1.11
dV	1.33	1.17
vG	1.38	1.11
R	1.34	1.06
Sch	1.32	1.17

Vergleichen wir die Tabellen 4 und 5 mit 6, so ersehen wir, dass zwischen der Größe der Täuschung bei monokularer und binokularer Beobachtung kein wesentlicher Unterschied besteht.

*Versuch 12.* Wie die Versuche 8 und 9, jetzt aber mit Fixation.

Als Fixationspunkt wählte ich einen leuchtenden Punkt mit einem Durchmesser von 4 mm, der in der Mitte des Feldes, also im Schnittpunkt der Bahnen, welche die beiden Punkte durchliefen, angebracht wurde.

TABELLE 7

Feldgröße  $15 \times 15$  cm,  $V_h$ : 10 cm/sec. Beobachtung mit Fixationspunkt

Bewegungsrichtungen	$\rightarrow \uparrow$	$\leftarrow \uparrow$
Vpn.	$V_h/V_v$	$V_h/V_v$
vdM	1.18	1.17
S	1.17	1.16
Li	1.26	1.25
M	1.30	1.25

Aus Tabelle 7 geht hervor, dass bei Fixation der Mitte der beiden Bewegungsbahnen der phänomenale Geschwindigkeitsunterschied zwischen der horizontalen und der vertikalen Bewegung herabgesetzt ist. Der phänomenale Geschwindigkeitsunterschied zwischen einer nach rechts und einer nach links gerichteten Bewegung ist jetzt aufgehoben.

Jetzt wollen wir untersuchen, wie es sich mit dem phänomenalen Ge-

geschwindigkeitsunterschied zwischen einer horizontalen und einer vertikalen Bewegung verhält, wenn die Lage des Körpers oder die Haltung des Kopfes nicht mehr vertikal ist. Dazu wurden die nachfolgenden Versuche angeordnet.

*Versuch 13.* Die Vp. lag auf der linken Seite auf einem Sofa und zwar so, dass die Verbindungslinie zwischen den Augen vertikal war. Die Mitte des Schirms war in Augenhöhe. Die übrigen Verhältnisse waren wie in den Versuchen 8 bis 10. Die Ergebnisse sind in Tabelle 8 zusammengestellt.

TABELLE 8  
In liegender Haltung.  $V_h$ : 10 cm/sec.

Bewegungsrichtungen	→ ↑	→ ↓	← ↑
Vpn.	$V_h/V_v$	$V_h/V_v$	$V_h/V_v$
vdM	0.81	0.81	0.85
W	1.11	1.00	1.00
Schl	0.97	0.89	0.98

Aus den Resultaten von Tabelle 8, und mehr noch aus den einzelnen Einstellungen der Vpn., geht hervor, dass man bei horizontaler Körperlage im Dunkeln seine normale Orientierung zum Teil verliert. Die Orientierung ist laut der Grösse der Geschwindigkeiten, die von den Vpn. als gleich beurteilt wurden, äusserst labil; bald scheint die normale, bald die Orientierung gemäss der liegenden Haltung, wobei also die objektive Horizontale die subjektive Vertikale wird, zu überwiegen. Es besteht eine grosse Unsicherheit über das Urteil, die in keinem der vorherigen Versuche vorkam. Dieselbe Geschwindigkeit wurde das eine Mal als viel zu langsam, sofort danach als viel zu schnell bezüglich der Standardgeschwindigkeit beurteilt.

Die Labilität der Orientierung geht auch aus dem Folgenden hervor. Die erste Vp. aus Tabelle 8 versuchte die Orientierung, wobei die objektive Horizontale als Vertikale aufgefasst wurde, beizubehalten. Dies gelang insofern, dass die Geschwindigkeit der Variablen in den verschiedenen Einstellungen sehr konstant war. Die Streuungen waren nämlich bzw. 0; 0.08 und 0. Es erfolgte aber ein leichtes Schwindelgefühl. Die Illusion war in diesem Falle der Illusion bei vertikaler Körperlage entgegengesetzt. Wenn man beachtet, dass der Zeitfehler auf die Illusionen in den beiden Situationen, d.h. in aufrechter und in liegender Haltung, einen entgegengesetzten Einfluss ausübt, so scheint die Annahme, dass die Grössen der



Illusionen in den beiden Körperlagen sich ziemlich genau entsprechen, berechtigt.

Hieraus geht also hervor, dass dasjenige was als horizontal oder vertikal aufgefasst wird, nicht von dem Augenstand abhängig ist, sondern durch die Bedeutung, die man den Hauptdimensionen des Raumes verleiht, bedingt wird. Bei unvoreingenommenen Vpn. überwiegt bald die eine, bald die andere Orientierung.

Aus Versuch 13 geht hervor, dass im Dunkeln in liegender Haltung eine gewisse Labilität der Orientierung besteht, wobei die Orientierung, bei der die objektive Vertikale als horizontal gesehen wird, vorzuherrschen scheint. Es fragt sich nun, wie sich dies im Hellen verhalten wird.

*Versuch 14.* Die Vpn. sollten die Geschwindigkeiten sowohl im Dunkeln wie auch unter einer schwachen Beleuchtung miteinander vergleichen. Die Beleuchtung war nicht so stark, dass man darin hätte lesen können; die Umrisse des Zimmers und die Gegenstände waren jedoch ganz deutlich wahrnehmbar. Die Vpn. lagen wie in Versuch 13 auf dem Sofa.

TABELLE 9

Vergleich zwischen Beobachtung im Dunkeln und im Hellen

	im Dunkeln	im Hellen
Bewegungsrichtungen	→ ↑	→ ↑
Vpn.	$V_h/V_v$	$V_h/V_v$
vdM	0.81	1.03
V	0.92	1.17
S	0.90	1.03

Vergleich der Quotienten  $V_h/V_v$  im Dunkeln und bei schwacher Beleuchtung ergibt also (Tab. 9), dass in liegender Haltung die normale Horizontal-Vertikalorientierung im Hellen besser beibehalten bleibt als im Dunkeln, jedoch auch nicht vollkommen. Was als horizontal oder vertikal aufgefasst wird, ist also nicht vom Augenstand abhängig, sondern wird durch die Struktur des ganzen Gesichtsfeldes mitbedingt.

*Versuch 15.* Es wurde untersucht, welcher der Einfluss auf den phänomenalen Geschwindigkeitsunterschied einer horizontalen und einer vertikalen Bewegung ist, wenn man diese unter einem Kopfstand von  $45^\circ$  seitwärts, und zwar entweder mit dem Kinn nach rechts oder nach links gerichtet, beobachtet.

Die Wahrnehmung war binokular, ohne Fixation.

In Tabelle 10 sind die Resultate zusammengestellt. Zum Vergleich wurden auch die Werte des Verhältnisses  $V_h/V_v$  bei normalem Kopfstand bestimmt.

TABELLE 10

Vergleich bei aufrechter und geneigter Kopfhaltung. Beobachtung binokular

Kopfhaltung	aufrecht		45° n.l.	45° n.r.
Bewegungsrichtungen	→↑	←↑	→↑	→↑
Vpn.	$V_h/V_v$	$V_h/V_v$	$V_h/V_v$	$V_h/V_v$
vdM	1.38	1.06	1.06	1.09
W	1.24	1.02	1.14	1.13
vBr	1.38	1.09	1.10	1.09

Versuch 16. Wie Versuch 15, die Wahrnehmung monokular (rechtes Auge).

TABELLE 11

Vergleich bei aufrechter und geneigter Kopfhaltung. Beobachtung monokular

Kopfhaltung	aufrecht		45° n.l.	45° n.r.
Bewegungsrichtungen	→↑	←↑	→↑	→↑
Vpn.	$V_h/V_v$	$V_h/V_v$	$V_h/V_v$	$V_h/V_v$
vdM	1.38	1.08	1.15	1.12
dV	1.33	1.17	1.13	1.14
Sch	1.32	1.17	1.06	1.08

Der Vergleich der Tabellen 10 und 11 ergibt an erster Stelle, dass es keinen Unterschied macht, ob der Kopf 45° nach links oder nach rechts gerichtet ist; auch zwischen monokularer und binokularer Beobachtung scheint kein wesentlicher Unterschied zu bestehen.

Es gibt einen geringen Unterschied in phänomenaler Geschwindigkeit zwischen der horizontalen und der vertikalen Bewegung; ein kleiner Unterschied ist aber schon auf Grund des Zeitfehlers zu erwarten.

Im allgemeinen neigten die Vpn. dazu, die beiden Bewegungen symmetrisch in Bezug auf den Kopf bewegen zu sehen, so dass die beiden Bewegungen nicht mehr als horizontal oder vertikal aufgefasst wurden. Man könnte hier also von einer egozentrischen Orientierung sprechen.

Versuch 17. Die sich bewegenden Punkte wurden auf die Decke projiziert. Über die rotierenden Spiegel wurde ein Schirm aus schwarzer Pappe

hingestellt, aus dem zwei Schlitze geschnitten wurden, so dass die beiden Bewegungsbahnen an der Decke einander in der Mitte kreuzten. Die Länge der beiden Bahnen an der Decke betrug 18 cm. Der Abstand der Decke bis zu den Spiegeln betrug 1.90 m. Die Geschwindigkeit der Bewegungen wurde nach der Zeitdauer der Bewegung über einen Abstand von 53 cm berechnet.

Die Vpn. lagen auf einem Sofa; die Bewegungen verliefen etwas hinter den Vpn.

In Tabelle 12 sind die Resultate erwähnt;  $V_v$  bedeutet die Geschwindigkeit der Bewegung parallel mit der Längsnachse des Körpers der Vpn.

TABELLE 12  
Bewegungen an der Decke. Beobachtung in liegender Haltung

Bewegungsrichtungen	→ ↑	→ ↓
Vpn.	$V_h/V_v$	$V_h/V_v$
vdM	1.38	1.37
D	1.56	1.33
vdK	1.31	1.10

Die phänomenale Geschwindigkeit der vertikalen Bewegung ist also (Tab. 12) beträchtlich grösser als die der horizontalen Bewegung. Bei den beiden letzten Vpn. aus Tabelle 12 scheint jetzt auch ein phänomenaler Geschwindigkeitsunterschied zwischen der steigenden (sich entfernenden) und der fallenden (sich nähernden) Bewegung vorzuliegen, und zwar wird die steigende Bewegung schneller als die fallende Bewegung gesehen. Im nächsten Abschnitt werden wir dies einer näheren Prüfung unterziehen.

*Versuch 18.* Die Versuchspersonen sassen jetzt aufrecht und beobachteten die Bewegungen, die schräg über und vor ihnen an der Decke erschienen.

TABELLE 13  
Bewegungen an der Decke. Beobachtung aufrecht sitzend

Bewegungsrichtungen	→ ↑	→ ↓
Vpn.	$V_h/V_v$	$V_h/V_v$
Li	1.04	1.00
	0.91	0.96
	1.00	1.03
vdM	0.78	0.90
	0.76	0.85
S	0.84	0.96
	0.79	0.90
Gl	1.50	1.31

In Tabelle 13 sind die Durchschnittswerte aus je 8 Einstellungen angeführt. Da die interindividuellen Ergebnisse nicht einstimmig waren und die Vpn. den Auftrag in diesem Falle als sehr schwierig empfanden, weil die Situation so labil war, wurden die Versuche einige Wochen später wiederholt. Dies machte aber keinen grossen Unterschied.

Als besondere Schwierigkeit dieses Versuches galt, dass man die Bewegungen bald an einer frontoparallelen Fläche, wobei die sich nähernde Bewegung als nach oben, die sich entfernende Bewegung als nach unten gerichtet gesehen wurde, bald an der Decke sah. Dies sind selbstverständlich die beiden Äussersten, zwischen denen die Lokalisation erfolgte. Sah man die Bewegungen an der Decke, so wurden sie eher als sagittal wie als vertikal gesehen. Sah man die Bewegungen an einer frontoparallelen Fläche oder zwischen diesen beiden Stellungen, so wurde die objektiv sich nähernde Bewegung als nach oben, die sich entfernende als nach unten gerichtet gesehen. Die Desorientierung war manchmal so gross, dass die Vp. meinte, dass die Richtung der Bewegung während des Versuches nicht konstant bliebe, sondern bald nach oben, bald nach unten verlief. Die Vp. S meinte — als nach Beendigung des Versuches die sich entfernende und die sich nähernde Bewegung nacheinander gezeigt wurden — dass beide nach oben gerichtet seien, die sich nähernde Bewegung nur etwas ausgeprägter.

Als Folge dieser Labilität und der daraus resultierenden Desorientierung traten weitere störende Phänomene auf. So wurde z.B. die Bewegung von links nach rechts nicht in konstanter Geschwindigkeit gesehen, obgleich dies wohl der Fall war. Bald wurde die Länge der horizontalen Bahn als länger, bald als kürzer als die vertikale, bzw. sagittale beurteilt. Weiter waren die Konstanzphänomene bedeutend beeinträchtigt.

#### 4. VERSUCHE ÜBER BEWEGUNGEN IN ENTGEGENGESetzten RICHTUNGEN

Die im vorigen Paragraphen erwähnten Versuche berechtigten uns einermassen zur Annahme, dass es einen Unterschied in phänomenaler Geschwindigkeit zwischen einer nach rechts und einer nach links gerichteten Bewegung gäbe. Dagegen wurde kein Unterschied zwischen einer aufsteigenden und einer absteigenden Bewegung gefunden. Um hierüber mehr Sicherheit zu bekommen, wollen wir in diesem Abschnitt die Bewegungen in entgegengesetzter Richtung auf direktem Wege miteinander vergleichen. An jedem dieser Versuche wurde von 10 Vpn. teilgenommen; von jeder Vp. wurden 6 Einstellungen erhalten.

Wir wollen hier auch den Einfluss des Zeitfehlers untersuchen, was in den vorigen Versuchen unterlassen wurde, zum Teil aus praktischen Gründen (zu lange Versuchszeit pro Vp.), zum Teil weil die gefundenen

Unterschiede derart gross waren, dass diese nicht nur als eine Folge des Zeitfehlers betrachtet werden konnten.

Aus technischen Gründen war der Zeitraum, in dem die beiden Bewegungen einander folgten, in diesen Versuchen etwas grösser als in den Experimenten über horizontale und vertikale Bewegung. Daher wird der hier gefundene Zeitfehler wahrscheinlich etwas grösser sein als derjenige, den man in den vorigen Versuchen in Abzug bringen müsste.

*Versuch 19a.* Feldgrösse  $15 \times 15$  cm, Punktdurchmesser 14 mm, Beobachtungsabstand 1.20 m. Geschwindigkeit der Standardbewegung  $V_s$ : 10 cm/sec. Die Wahrnehmung war binokular, ohne Fixation.

Die Standardbewegung war von rechts nach links, die Variable von links nach rechts.

*Versuch 19b.* Der Standard von links nach rechts, die Variable von rechts nach links.

TABELLE 14

Vergleich von zwei horizontalen Bewegungen in entgegengesetzter Richtung.  
 $V_s$ : 10 cm/sec.

Standardrichtung	nach l.	nach r.	$V_r/V_l$ durchschn.
Vpn.	$V_r/V_s$	$V_s/V_l$	
vdM	0.99	1.07	1.03
Li	0.97	1.08	1.02
S	1.03	1.11	1.07
Kl	0.99	1.07	1.03
dJ	0.91	1.12	1.01
Wr	0.96	1.12	1.03
Br	0.93	1.02	0.98
R	0.99	1.14	1.06
K	0.95	1.01	0.98
P	1.00	1.11	1.06
durchschnittlich	0.97	1.09	1.03

Die Ergebnisse laut Tabelle 14 entsprechen aber nicht der Erwartung, dass nämlich die nach links gerichtete Bewegung phänomenal beträchtlich schneller wäre. Nach Berechnung des geometrischen Mittels, nachdem der Zeitfehler also in Abzug gebracht ist, findet man nur einen Unterschied von 3 %. Zwar ist der Unterschied gesichert,  $\sigma = 3\%$ ,  $\sigma_M = 1\%$  und  $t = 3.0$ , aber dieser ist doch beträchtlich geringer als man auf Grund der vorherigen Versuche erwarten könnte und gilt überdies nicht für alle Vpn.

Die Versuche 19a und 19b wurden jetzt in umgekehrter Folge wiederholt und zwar mit 3 Vpn. des vorigen Versuches und mit 7 neuen Vpn.

*Versuch 19c.* Der Standard bewog von links nach rechts, die Variable von rechts nach links.

*Versuch 19d.* Der Standard bewog von rechts nach links, die Variable von links nach rechts.

TABELLE 15

Vergleich von zwei horizontalen Bewegungen in entgegengesetzter Richtung.  
 $V_s$ : 10 cm/sec.

Standardrichtung	nach r.	nach l.	$V_r/V_l$ durchschn.
Vpn.	$V_s/V_l$	$V_r/V_s$	
vdM	1.24	1.04	1.14
Li	1.19	0.96	1.07
S	1.16	1.09	1.12
Schw	1.23	1.00	1.11
V	1.23	1.00	1.11
W	1.29	1.00	1.13
F	1.23	0.99	1.10
N	1.16	1.03	1.10
B	1.24	1.00	1.11
Sp	1.20	0.98	1.08
durchschnittlich	1.22	1.01	1.11

Die Ergebnisse der Tabelle 15 scheinen dem zuvor gefundenen Unterschied zwischen der nach links und der nach rechts gerichteten Bewegung besser zu entsprechen. Nach Abzug des Zeitfehlers gibt es einen phänomenalen Geschwindigkeitsunterschied von 11 %. Dieser Unterschied ist gesichert, denn  $\sigma = 2\%$ ,  $\sigma_M = 0.7\%$  und  $t = 15.7$ .

Zwischen den neuen Vpn. und den Vpn., die auch an den Versuchen 19a und 19b teilnahmen, liegt kein Unterschied vor. Die Frage ist nun, wie soll der Unterschied der Ergebnisse nach Tabelle 14 und 15 (1.03 gegenüber 1.11) erklärt werden und welcher Unterschied ist der richtige? Auf Grund der bei dem Vergleich einer horizontalen und einer vertikalen Bewegung erhaltenen Ergebnisse, scheint es am wahrscheinlichsten, dass die Ergebnisse der Tabelle 15 richtig sind, obgleich ein grösserer Unterschied von ca. 20 % zu erwarten wäre. Der direkte Vergleich zwischen einer nach links und einer nach rechts gerichteten Bewegung ergibt also einen geringeren phänomenalen Geschwindigkeitsunterschied als der indirekte Vergleich, bei dem man die vertikale Bewegung in Beziehung zu den beiden entgegengesetzten

horizontalen vergleicht und man das Verhältnis zwischen der Geschwindigkeit der nach links und nach rechts gerichteten Bewegungen bei subjektiver Gleichheit berechnen könnte. Im letzten Falle ergab sich ein Unterschied in der Größenordnung von etwa 20 %.

Welches Resultat aber am richtigsten ist, das nach Tabelle 14 oder 15, soll einer näheren Prüfung unterzogen werden, und man kann sich abfragen, was geschehen würde, wenn man die Versuche der Tabellen 14 und 15 fortsetzen würde. Mit 2 Vpn. wurden nun die Versuche fortgesetzt, und zwar so, dass an einem Tage die Versuche 19a und 19b, am anderen Tage die Versuche 19c und 19d wiederholt wurden. Die Durchschnittswerte von 8 Einstellungen pro Tag für jede der beiden Vpn. sind in Tabelle 16 erwähnt, in der auch die Werte aus den Tabellen 14 und 15 aufgenommen sind.

TABELLE 16

Fortsetzung der Versuche 19 bei zwei Vpn. an verschiedenen Tagen

Versuch 19:		a	b		c	d
Standardrichtung		←	→		→	←
Vpn.	Tag	$V_r/V_l$	$V_r/V_l$	Tag	$V_r/V_l$	$V_r/V_l$
Li	1.	0.97	1.08	2.	1.19	0.96
	3.	0.97	1.08	4.	1.21	0.97
	5.	0.99	1.14	6.	1.17	0.97
	7.	1.02	1.17	8.	1.15	1.01
S	1.	1.03	1.11	2.	1.16	1.09
	3.	1.03	1.12	4.	1.16	1.09
	5.	1.08	1.17	6.	1.17	1.09
	7.	1.08	1.17	8.	1.17	1.09

Aus Tabelle 16 geht nun hervor, dass — falls die Versuche fortgesetzt werden — das Ergebnis der Versuche a und b dem der Versuche c und d völlig entspricht. Bei Vp. Li sind nach drei Wiederholungen die Quotienten der Versuche a und d bzw. 1.02 und 1.01, die der Versuche b und c bzw. 1.17 und 1.15. Bei Vp. S sind diese Quotienten nach drei Wiederholungen für a und d bzw. 1.08 und 1.09 und für b und c beide 1.17. Die Annahme, dass unter den hier geltenden Verhältnissen die nach links gerichtete Bewegung phänomenal schneller ist als die nach rechts gerichtete (ca. 11 %), scheint also berechtigt.

Wir wollen jetzt durch einen direkten Vergleich der aszendenten und deszendenten Bewegung prüfen, ob es tatsächlich keinen Geschwindigkeits-

unterschied zwischen den beiden entgegengesetzten Richtungen der vertikalen Dimension gibt. Dazu wurden die nachstehenden Versuche vorgenommen.

*Versuch 20 a.* Die Bedingungen waren wie in Versuch 19.  $V_s$ : 10 cm/sec.

Die Standardbewegung war von oben nach unten; die Variable von unten nach oben.

*Versuch 20 b.* Die Standardbewegung war von unten nach oben; die Variable von oben nach unten.

TABELLE 17

Vergleich von zwei vertikalen Bewegungen in entgegengesetzter Richtung.  
 $V_s$ : 10 cm/sec.

Standardrichtung	$s = d$	$s = a$	durchschn.
Vpn.	$V_d/V_a$	$V_d/V_a$	$V_d/V_a$
W	1.16	0.89	1.02
Dr	1.12	1.00	1.06
K	1.27	1.01	1.13
vdS	1.09	1.04	1.06
Tr	0.91	0.99	0.95
Mi	0.98	0.92	0.95
P	1.00	1.02	1.01
vB	1.11	0.85	0.97
K	0.99	0.91	0.95
B	1.03	1.00	1.01
durchschnittlich	1.07	0.96	1.01

Der Erwartung gemäss gibt es keinen phänomenalen Geschwindigkeitsunterschied zwischen einer aszendenten und einer deszendenten Bewegung. Das Verhältnis  $V_d/V_a$  nach Abzug des Zeitfehlers beträgt 1.01 (Tab. 17).

Da aus dem Vergleich der Geschwindigkeiten in den beiden horizontalen Richtungen hervorgegangen war, dass die Grösse des ermittelten Geschwindigkeitsunterschiedes eventuell von der Expositionsfolge abhängig sein könnte, wurden die Versuche 20a und b auch noch in umgekehrter Folge wiederholt.

*Versuch 20 c.* Die Standardbewegung war in aszender, die Variable in deszender Richtung.

*Versuch 20 d.* Die Standardbewegung war in deszender, die Variable in aszender richtung.



TABELLE 18

ich von zwei vertikalen Bewegungen in entgegengesetzter Richtung.  
 $V_s$ : 10 cm/sec.

Standardrichtung	$s = a$	$s = d$	durchschn.
Vpn.	$V_d/V_a$	$V_d/V_a$	$V_d/V_a$
dJ	0.94	1.14	1.03
F	1.00	0.93	0.97
dV	1.00	1.00	1.00
S	0.95	1.07	1.01
vdM	1.02	0.97	0.99
Bo	1.00	0.90	0.95
vBa	0.87	0.92	0.89
Dr	0.87	1.10	0.98
Kl	0.88	1.07	0.97
vBr	1.00	1.06	1.03
durchschnittlich	0.95	1.02	0.98

Der Durchschnittswert des Verhältnisses  $V_d/V_a$  laut Tabelle 18 beträgt 0.98. Die Ergebnisse der Tabellen 17 und 18 kann man als übereinstimmend betrachten und die Annahme scheint berechtigt, dass es unter den hier geltenden Verhältnissen keinen phänomenalen Geschwindigkeitsunterschied zwischen einer aszendenten und einer deszendente Bewegung gibt.

Bei der Projizierung einer horizontalen und einer vertikalen Bewegung an der Decke (Versuch 17) scheint ein phänomenaler Geschwindigkeitsunterschied zwischen den Bewegungen in entgegengesetzter Richtung der Vertikalen, bzw. den Bewegungen, parallel der Längsachse des Körpers, vorzuliegen. Dies wollen wir jetzt näher prüfen, indem wir die Geschwindigkeiten der Bewegungen in diesen beiden Richtungen miteinander vergleichen.

*Versuch 21.* Die Bewegungen wurden auf die Decke projiziert; die Vp. lag auf einem Sofa; die Verhältnisse waren wie in Versuch 17.  $V_s$ : 10 cm/sec.

- Die Standardbewegung verlief in aszendenter (sich von der Vp. entfernender), die Variable in deszendenter (sich der Vp. nähernder) Richtung;
- Die Standardbewegung verlief in deszendenter (sich der Vp. nähernder), die Variable in entgegengesetzter Richtung.

Aus Tabelle 19 geht also tatsächlich hervor, dass die aszendente, bzw. die sich von der Vp. entfernende Bewegung, phänomenal beträchtlich schneller ist als die deszendente, bzw. die sich der Vp. nähernde Bewegung.

TABELLE 19

Vergleich von an der Decke projizierten Bewegungen in entgegengesetzter Richtung. Beobachtung in liegender Haltung.  $V_s$ : 10 cm/sec.

Standardrichtung	$s = a$	$s = d$
Vpn.	$V_d/V_a$	$V_d/V_a$
V	1.58	1.45
vdS	1.41	1.42
B	1.40	1.46

Versuch 22. Wie Versuch 21; die Vp. sass aber jetzt aufrecht; die bewegendenden Punkte erschienen schräg vor ihr an der Decke.

TABELLE 20

Bewegungen an der Decke. Beobachtung aufrecht sitzend

Standardrichtung	$s = d$	$s = a$
Vpn.	$V_d/V_a$	$V_d/V_a$
P	1.12	1.10
R	1.02	0.98
W	1.09	0.98

Nach Tabelle 20 scheint jetzt wenig oder gar kein Unterschied zwischen der ascendenten und der descendenten Bewegung vorzuliegen.

Aus den bisherigen Versuchen ersehen wir, dass die phänomenale Geschwindigkeit von Bewegungen in den verschiedenen Richtungen unseres Wahrnehmungsraumes bei gleicher objektiver Geschwindigkeit nicht immer dieselbe ist. Eines der überraschendsten Ergebnisse ist, dass der Geschwindigkeitsunterschied zwischen einer horizontalen und einer vertikalen Bewegung, der von Brown genannt und seitdem überall in der Literatur erwähnt wird, nur gilt, falls die horizontale Bewegung von links nach rechts verläuft. Die Bewegungen nach links und nach rechts weisen einen Unterschied von ca. 11 % auf, während die Bewegung von rechts nach links schneller gesehen wird. Diese Ergebnisse lassen sich schwer erklären, da Rechts- und Linkshändigkeit, bzw. Rechts- und Linksausgigkeit, keinen Einfluss darauf hat.

Bewegungen in verschiedenen Richtungen haben aber auch eine gewisse Bedeutung für uns, die mit unserer Organisation und unserem Orientierungssystem in Zusammenhang steht. Dass die beiden Richtungen

der horizontalen Dimension eine verschiedene Bedeutung haben, wollen wir in den beiden nachstehenden Versuchen untersuchen. Die Anzahl Vpn. betrug in beiden Fällen 42.

*Versuch 23.* Die Vpn. wurden gebeten, ein ausfahrendes Schiff zu zeichnen.

*Versuch 24.* Der Auftrag lautete: Stellen Sie sich vor, dass Sie spazieren gehen und dass Sie unterwegs Ihrem Freunde begegnen. Zeichnen Sie nun ein Bild Ihres Freundes.

In Tabelle 21 sind die Ergebnisse dieser beiden Versuche aufgezeichnet.

TABELLE 21

Bedeutung der entgegengesetzten Richtungen in der horizontalen Dimension bei Zeichenversuchen

	gezeichnete Richtung			
	→		←	
	n	%	n	%
ausfahrendes Schiff	32	76	10	24
Person, der man begegnet	7	17	35	83

27 der 42 Personen oder 64 % zeichneten das ausfahrende Schiff nach rechts und die Person, der man begegnet, nach links gerichtet; 2 Personen oder 5 % zeichneten beide in entgegengesetzter Richtung; 13 Personen oder 31 % zeichneten das Schiff und die Person in der gleichen Richtung. Von diesen letzten muss man annehmen, dass die Bewegungsrichtung für sie keine Bedeutung hat, oder dass sie sich nicht in die Aufgabe vertieft haben und die Richtung ihrer Zeichnung vom Zufall bestimmt wurde. Auch in den beiden erstgenannten Gruppen werden sich vermutlich einige Personen befinden, deren Ergebnisse vom Zufall bestimmt sind, so dass der Umstand, dass bei 2 Vpn. ein Ergebnis, der Erwartung entgegengesetzt, gefunden wird, keine Bedeutung hat. Es lag kein Unterschied zwischen Rechts- und Linkshändigkeit vor; die beiden Vpn., die ein entgegengesetztes Resultat lieferten, waren rechtshändig.

Der Unterschied zwischen den 27 Vpn., für die die ausfahrende Bewegung nach rechts gerichtet ist, die nähernde nach links, und die 2 Vpn. für die diese Richtungen umgekehrt verlaufen, ist wesentlich gesichert ( $t = 5.7$ ;  $P < 0.001$ ).

Aus diesen Versuchen scheint die Annahme berechtigt, dass im allgemeinen eine nach rechts gerichtete Bewegung die Bedeutung einer Entfernung, eine nach links gerichtete Bewegung die Bedeutung einer Näherung hat.

## 5. ZUSAMMENFASSUNG DER ERGEBNISSE

1. Bei einer Feldgrösse von  $46.2 \times 46.2$  cm und  $30 \times 30$  cm und einem Punktdurchmesser von bzw. 40 und 30 mm, gibt es einen Unterschied in phänomenaler Geschwindigkeit zwischen einer vertikalen und einer *nach links* gerichteten Bewegung (s. Tabellen 1, 2 und 3).

a. Es scheint von der Geschwindigkeit der bewegenden Punkte abhängig zu sein, ob die horizontale oder die vertikale Bewegung schneller ist.

b. Es hat den Anschein, dass es zwei Typen Vpn. gibt: der eine sieht bei einer Geschwindigkeit von 5 cm/sec. die nach links gerichtete, horizontale Bewegung bei gleicher objektiver Geschwindigkeit schneller, bei einer Geschwindigkeit von 20 cm/sec. die vertikale Bewegung schneller; bei dem anderen Typ ist dieses Verhältnis gerade umgekehrt.

2. Bei einer Feldgrösse von  $15 \times 15$  cm und einem bewegenden Punkt von 14 mm  $\emptyset$ , ist mit freien Augenbewegungen die phänomenale Geschwindigkeit der vertikalen Bewegung ca. 30 % grösser als die der *nach rechts* gerichteten, horizontalen Bewegung.

Hinsichtlich der nach links gerichteten, horizontalen Bewegung gibt es wenig oder gar keinen Unterschied (Tab. 4 und 5).

3. Eine nach links gerichtete, horizontale Bewegung ist bei gleicher objektiver Geschwindigkeit phänomenal ca. 11 % schneller als eine nach rechts gerichtete (Tab. 15).

4. Es gibt keinen Unterschied in phänomenaler Geschwindigkeit zwischen den beiden Richtungen der vertikalen Dimension (Tab. 17 und 18).

5. Es macht keinen wesentlichen Unterschied ob die Beobachtung monokular oder binokular ist.

6. Unter Fixation der Mitte der beiden Bewegungsbahnen wird der phänomenale Geschwindigkeitsunterschied zwischen der vertikalen und der horizontalen Bewegung beträchtlich herabgesetzt. Der Unterschied zwischen einer nach rechts und einer nach links gerichteten Bewegung wird bei strenger Fixation aufgehoben (Tab. 7).

7. In liegender Haltung im Dunkeln besteht eine Unsicherheit über die Orientierung nach den Hauptdimensionen unseres Wahrnehmungsraumes. Besonders am Anfang ist die Labilität der Orientierung gross, wobei bald die alte Orientierung nach der objektiven Vertikalen, bald die gemäss der Stellung des Körpers überwiegt (Tab. 8).

8. In liegender Haltung unter schwacher Beleuchtung wird die normale Orientierung leichter beibehalten als im Dunkeln (Tab. 9).

9. Bei einer Kopfhaltung von  $45^\circ$  seitwärts nach links oder nach rechts, werden die horizontale und die vertikale Bewegung überwiegend symmetrisch unter einem Winkel von  $45^\circ$  hinsichtlich des Kopfes wahrgenommen, wobei also die Kopfhaltung als Referenzsystem dient. Es lag jetzt kein Unterschied in phänomenaler Geschwindigkeit vor (Tab. 10 und 11).

10. Wenn die Bewegungen an der Decke projiziert werden, etwas hinter der Vp., die in liegender Haltung die Bewegungen beobachtet, gibt es einen beträchtlichen Unterschied in phänomenaler Geschwindigkeit zwischen der horizontalen und der vertikalen Bewegung. Überdies ist jetzt die ascendente Bewegung phänomenal schneller als die descendente (Tab. 12 und 19).

11. Wenn die Vp. aufrecht sitzend die Bewegungen schräg vor ihr an der Decke wahrnimmt, gibt es wenig oder gar keinen Unterschied in phänomenaler Geschwindigkeit zwischen der sich nähernden und der sich entfernenden Bewegung. Für einige Vpn. ist die horizontale Bewegung phänomenal schneller als die vertikale, bzw. sagittale Bewegung, für andere macht es keinen Unterschied oder ist die vertikale bzw. sagittale Bewegung schneller. Die interindividuellen Differenzen sind gross und hängen wahrscheinlich zusammen mit der Weise, wie man die Bewegungen lokalisiert, d.h. in einer frontalparallelen Fläche oder an der Decke. Die Vpn. fühlten sich ziemlich desorientiert (Tab. 13 und 20).

## 6. VERSUCH ZUR DEUTUNG DER RESULTATE

Im ersten Paragraphen haben wir uns die Frage vorgelegt, ob der phänomenale Geschwindigkeitsunterschied zwischen einer objektiv vertikalen und horizontalen Bewegung bei Veränderung der Haltung des Kopfes oder der Stellung des Körpers beibehalten bleibe? In den vorherigen Versuchen haben wir gesehen, dass dies nicht der Fall ist. Ebenso wenig tritt in liegender Haltung, wenn die Lage des Körpers also um  $90^\circ$  gedreht ist, eine Umkehrung dieses Unterschiedes auf, was der neuen Lage des Körpers und dem Augenstand entsprechen würde.

Wenn auch physiologische Veränderungen Motive für die Erfassung des Wahrnehmbaren liefern<sup>1</sup>, so ist doch die Bedeutung der Haupttrichtungen

<sup>1</sup> Die Versuche von R. P. Mesker (De optische localisatie onder invloed van optische en houdingsfactoren, Amsterdam, 1953) zeigen eine Beziehung der optischen Lokalisation zur Körperhaltung. Der Autor basiert sich auf der innervationstonustheorie von C. O. Roelofs (Die optische Lokalisation, Arch. Augenheilk. 109, 1936)

des visuellen Wahrnehmungsraumes, bis zu einem gewissen Grade, von dem Entwurf, den wir wählen, abhängig. Wie könnte man z.B. physiologisch erklären, dass die Geschwindigkeitstäuschung sich völlig umkehrt, wenn man in liegender Haltung die objektiv horizontale Bewegung als eine vertikale auffasst, d.h. in dem sich objektiv horizontal bewegendem Punkt einen fallenden oder steigenden Punkt sieht, während dies bei unvoreingenommener, naiver Beobachtung keineswegs der Fall ist?

Unser visueller Wahrnehmungsraum besitzt in der horizontalen, der vertikalen und der sagittalen Dimension ein Referenzsystem, auf das wir unsere Wahrnehmungen beziehen. Der Wert, den die drei Dimensionen für dieses Referenzsystem haben, ist aber nicht gleichwertig, sondern u.a. davon abhängig, ob wir ein Objekt in Ruhe oder in Bewegung, im Hellen oder im Dunkeln beobachten. Vor allem aber wird deren Wert dadurch bestimmt, dass wir selber daran teilhaben, selber dieses Referenzsystem mitkonstituieren und ihm eine bestimmte Bedeutung gewähren. Die Lage eines Gegenstandes im Raum wird nicht nur von den drei Dimensionen des objektiven Raumes als Referenzsystem bestimmt, aber diese Lage erhält erst ihre Konsolidation durch die Bedeutung, die ich dem Referenzsystem verleihe, und zwar aus der Relation, die zwischen mir und dem Referenzsystem besteht.

Ob wir eine Bewegung als horizontal oder vertikal auffassen, wird nicht nur von der Horizontalen oder der Vertikalen des objektiven Raumes bestimmt, ebensowenig nur durch die Lage, die der Körper oder die Augen hinsichtlich der Richtung der Bewegung einnehmen, sondern durch die Bedeutung, die das Referenzsystem, das zusammen mit mir eine Einheit bildet, für mich hat. Wenn die Lage des Körpers bezüglich der objektiven Vertikalen verändert wird, verändert damit nicht im gleichen Masse die Lage der optischen Vertikalen. Was ich horizontal oder vertikal nenne, korrespondiert nicht nur mit der Lage meines Körpers oder meiner Augen; fällt also nicht zusammen mit den horizontalen, bzw. vertikalen Retinameridianen. Ebensowenig behauptet sich die ursprüngliche Lage. Welcher Dimension ich in liegender Haltung die Bedeutung vertikal gewähre, wird hauptsächlich von den anderen Motiven, die meinen Raum mitkonstituieren, bedingt werden: die Gegenstände, die sich darin befinden, die für mich eine Bedeutung haben, in der die Bedeutung des ursprünglichen Referenzsystems mitgegeben ist und die jetzt ihrerseits als Referenzsystem für meine Orientierung dienen. Daher wird man im Dunkeln die ursprünglich objek-

und erklärt die Stellung der optischen Vertikalen unter Einfluss von optischen und Haltungsfaktoren aus Änderungen des reflektorischen Blicktonus und des reflektorischen Innervationstonus der Körpermuskulatur.

tive Vertikale vorzugsweise als mehr horizontal gerichtet auffassen. Im Hellen behauptet die objektive Vertikale sich leichter als optische Vertikale, da die Gegenstände, die ich wahrnehme, dann als Hauptmotive zur Konstitution meines Raumes funktionieren und die ursprüngliche Orientierung stabilisieren.

Eine gewisse Labilität in der Orientierung wird sich, besonders im Anfang nach einer Lageänderung des Körpers, geltend machen, wenn auch kein festes Referenzsystem mit einer neuen Bedeutung hergestellt ist. Ich besitze aber innerhalb gewisser Grenzen die Freiheit, an dieser Sinnverleihung einen aktiven Anteil zu nehmen. Die phänomenale Richtung der Bewegungen wird bestimmt durch die Bedeutung, die ich ihr verleihe, ungeachtet ob ich mich dabei mehr passiv oder aktiv verhalte.

Aus den Versuchen im Paragraphen 4 ergab sich, dass die nach links gerichtete Bewegung bei objektiver Gleichheit schneller gesehen wird als die nach rechts gerichtete. Weiter sahen wir, dass man der nach rechts gerichteten Bewegung die Bedeutung von Ausfahren oder Entfernen, der nach links gerichteten die von Begegnen, Nähern beimisst. Die Bedeutungen unserer Welt und unsere Organisation sind immer korrelativ aufeinander bezogen. Den Raum als bewohnbaren Raum, als einen Raum in dem die Dinge ihren Platz und ihre Bedeutung haben, konstituiere ich, indem ich mich in ihn begeben, und ich vollziehe virtuell die Bewegung mit, die ich wahrnehme. Nun ist der Mensch ein expansives Wesen, ein Wesen, das „ausfährt“ und nicht an einem Ruhepunkte verweilt. Er befindet sich unterwegs und seine natürliche Bewegungsrichtung — und diese ist zugleich die Richtung, in der er sich orientiert — scheint, insofern es die horizontale Dimension betrifft, nach rechts gerichtet zu sein. *Ich möchte nun die Hypothese vorschlagen, dass wir dadurch, dass wir uns unterwegs befinden und zwar nach rechts gerichtet, die Geschwindigkeit einer Bewegung in derselben Richtung als unsere Orientierungsrichtung unterschätzen und diejenige der Bewegung, die uns phänomenal begegnet, überschätzen.*<sup>2</sup>

Die Menschheit hat von alters her einen verschiedenen Sinn mit rechts und links verbunden. Diese Verschiedenheit kommt noch in zahlreichen Gebräuchen und Ausdrücken zur Äusserung. Meistens wird mit rechts der Sinn des Guten verbunden, bisweilen aber auch mit links. Hierin besteht

<sup>2</sup> Ich befasse mich momentan mit einer Anzahl Versuchen, um diesen Zusammenhang näher zu begründen. Es hat z.B. den Anschein, dass die Kopfbewegungen nach rechts leichter und schneller seien als die nach links. Die Versuche befinden sich aber noch nicht in einem Stadium, dass deren Resultate veröffentlicht werden könnten.



wenig Übereinstimmung. Auch im Altertum findet man in zahlreichen kulturellen Gebräuchen diesen Unterschied zwischen rechts und links. Bachofen weist darauf hin, dass in alten matriarchalen Kulturen die linke Hälfte des Körpers die bevorzugte Seite war, während dagegen in patriarchalen Kulturen die rechte Seite den Vorzug hatte. Links wird mit dem Weiblichen, Passiven, rechts mit dem Männlichen, Aktiven verbunden. Im Indogermanischen heisst die rechte Hand männlich, die linke weiblich (Thurnwald).<sup>3</sup>

Auch in der psychoanalytischen Literatur hat man dem Problem Rechts-Links Aufmerksamkeit gewidmet. Rechts ist bei den Patienten meistens ein Symbol des Guten, doch auch wohl links. Nach Rümke<sup>4</sup> sei hier jedoch nicht primär die Schätzung vom Guten oder vom Bösen, diese Sinnverleihung sei sekundär und von der Lebensgeschichte abhängig, sondern handele es sich um die Würdigung des Weiblichen und des Männlichen, wobei links das Weibliche, rechts das Männliche vertrete. Bei Linkshändern wäre dieser Unterschied umgekehrt.

Die Asymmetrie, die wir in der Schätzung der Geschwindigkeit nach rechts und links gefunden haben, ist jedoch nicht von Rechts- oder Linkshändigkeit abhängig, sei es, dass vermutlich wohl ein geringer Unterschied vorliegt und die Täuschung bei Rechtshändern etwas grösser ist. Mit einer Unterscheidung in eine männliche oder aktive und eine weibliche oder passive Hälfte unserer Organisationsstruktur, die von Links- und Rechtshändigkeit, bzw. Links- und Rechtsäugigkeit abhängig wäre, ist jedoch der Kern unseres Problems nicht berührt. Für die Beurteilung der Geschwindigkeit in der horizontalen Dimension scheint mir primär unsere eigene Bewegungsgerichtetheit bestimmend zu sein, die man vielleicht als eine jedem Verhalten vorgegebene Orientierung unseres In-der-Welt-Seins zu verstehen hat.<sup>5</sup>

In der vertikalen Dimension bestehen keine Differenzen in phänomenaler Geschwindigkeit zwischen den beiden entgegengesetzten Richtungen. Dies wird deutlich, wenn wir erwägen, dass unsere Stellung hinsichtlich der Horizontalen und der Vertikalen radikal anders ist. Kraft unserer Organisation und der Gravitation, der wir unterworfen sind, sind wir von oben nach unten gerichtet, doch wir haben in dieser Gerichtetheit den Endpunkt als Ruhepunkt erreicht. Dadurch dass wir in der vertikalen Dimension einen

<sup>3</sup> Zit. nach H. C. Rümke: *Studies en Voordrachten*, 1943. S. 371.

<sup>4</sup> H. C. Rümke: *Rechts en Links in de Psychopathologie*, *Studies en Voordrachten*, 1943.

<sup>5</sup> Weitere Untersuchungen, etwa bei Völkern, deren Schrift von rechts nach links ausgeführt wird, könnten möglicherweise ein neues Licht auf dieses Problem werfen.



Ruhepunkt erreicht haben, ist die Bewohnbarkeit unseres Raumes gesichert, und sind wir imstande, unsere Stabilität zu behaupten. In der horizontalen Ebene dagegen befinden wir uns unterwegs. Da wir uns in vertikaler Richtung phänomenal nicht bewegen, wird die Geschwindigkeit in aufwärtser Richtung nicht überschätzt.

Wenn wir in liegender Haltung die Bewegungen über und schräg hinter uns beobachten, gibt es wohl einen Unterschied in phänomenaler Geschwindigkeit zwischen der nach oben und der nach unten gerichteten Bewegung, wie wir ersehen haben. Da ich die Bewegung, die ich wahrnehme, virtuell mit vollziehe, hat meine Bewegung in der abwärtsen Richtung phänomenal den Endpunkt als Ruhepunkt noch nicht erreicht. Dies bildet für uns eine unnatürliche Situation, eine Situation, deren Stabilität aufgehoben ist und die nicht bewohnbar ist. Dies möge auch aus dem Umstand hervorgehen, dass in dieser Situation die Konstanzphänomene in der Organisation unserer Wahrnehmung beträchtlich verringert sind: Form- und Grössenkonstanz sind grösstenteils aufgehoben.

Eine weitere Unterstützung für die hier entwickelte Hypothese dürfte der Umstand geben, dass bei strenger Blickfixation der Unterschied in phänomenaler Geschwindigkeit zwischen einer Bewegung nach rechts und nach links ausgeglichen ist.

Die Hypothese, dass unsere Orientierungsrichtung in der vertikalen Dimension von oben nach unten läuft, könnte man vermutlich als im Einklang mit der Bodenständigkeitshypothese von De Boer<sup>6</sup> und den Auffassungen Benussis betrachten, nach dem *ambiguous figures* vorzugsweise gesehen werden, als ob man sie von oben herab betrachtet. Nach Corkill und Lythgoe<sup>7</sup> wären die Augenbewegungen von oben nach unten leichter und schneller als von unten nach oben. Auch dies könnte im Einklang mit unserer natürlichen Gerichtetheit von oben nach unten sein.

Der phänomenale Geschwindigkeitsunterschied zwischen einer horizontalen und einer vertikalen Bewegung ist, unter Beibehaltung des Prinzips, dass die Geschwindigkeit in der Richtung der natürlichen Orientierung unterschätzt wird, leicht zu erklären.

Dem hier entwickelten Gedankengang entsprechend, sind zwischen Bewegungen in verschiedenen Richtungen qualitative Differenzen zu erwarten. Befragt man die Vpn. ob sie zwischen zwei Bewegungen in verschiedenen Richtungen qualitative Verschiedenheiten angeben könnten, so

<sup>6</sup> De Boer: Über umkehrbare Zeichnungen, Arch. ges. Psychol. 18, 1910.

<sup>7</sup> Corkill and Lythgoe: Some experiments on eye-movements. Brit. J. Ophthalm. 13, 1929.

wurde einstimmig die vertikale Bewegung als eine aktivere gegenüber der horizontalen beurteilt, was der subjektiven Erlebung einer grösseren Geschwindigkeit entspricht.

Die Verschiedenheit zwischen der nach rechts und der nach links laufenden Bewegung war schwieriger zu beschreiben. In diesem Falle wurden zwei Differenzen erwähnt. Die eine war, dass die Bewegung nach links mehr Widerstand hervorrief und dadurch subjektiv ein grösseres Aktivitätsgefühl bewirke. Für einige Linkshänder war es aber die Bewegung nach rechts, die ein Gefühl grösseren Widerstandes hervorrief, so dass hiermit nicht die wesentliche Differenz gekennzeichnet ist. Die andere Verschiedenheit, die auch für Linkshänder galt, war, dass die nach rechts gerichtete Bewegung den Eindruck einer passiven, gleichmässig weiterfliessenden Bewegung machte, während die nach links gerichtete Bewegung einen abrupteren Charakter hatte.

Von der nach oben und nach unten gerichteten Bewegung gelang es nicht von den Vpn. eine Beschreibung irgendeiner Differenz zu erhalten. Auf einem grösseren Felde und bei einer Geschwindigkeit von 5 cm/sec. wurde aber eine Asymmetrie zwischen den beiden Hälften des Feldes wahrgenommen: die Bewegung über die untere Hälfte war phänomenal schneller als über die obere Hälfte. Unter diesen Umständen hätte es wahrscheinlich wohl einen Unterschied in phänomenaler Geschwindigkeit zwischen der aufsteigenden und der absteigenden Bewegung gegeben. Dies wurde aber nicht geprüft. Die Absicht war, die Versuche unter möglichst reduzierten Verhältnissen (im Dunkeln, kurze Expositionszeit) vorzunehmen, da dies die günstigsten Bedingungen schafft, die ursprünglichen Orientierungstendenzen zu zeigen.

Nachdem ich meine Versuche beendet hatte, kam mir das Buch von Martti Takala: „Asymmetries of the visual space“ (Helsinki 1951), in die Hände. Die von Takala gefundenen Asymmetrien in der Organisation unseres visuellen Wahrnehmungsraumes entsprechen den in meiner Untersuchung gefundenen Bewegungsasymmetrien. Auf Grund zahlreicher Versuche über Lokalisierung, Erinnerung und Wiedererkennung kommt er zur Folgerung, dass unser optischer Wahrnehmungsraum asymmetrisch ist und die linke und obere Hälfte bevorzugt werden. Auch in der Weise, in der wir unsere Wahrnehmungen organisieren, gibt es Vorzugsrichtungen: ein Objekt in Ruhe wird leichter wahrgenommen und wiedererkannt, wenn es nach links oder nach oben gerichtet ist als nach rechts bzw. nach unten. Die Vorzugsrichtungen eines statischen Objektes sind also horizontal von rechts nach links, vertikal von unten nach oben und sagittal auf uns hin gerichtet. Bei der Beurteilung der Geschwindigkeit einer Bewegung ist die

Asymmetrie derjenigen der statischen Objekte gleichsam entgegengesetzt. Bewegung nach links, nach oben und auf uns hingerichtet, werden überschätzt. Takala betrachtet dies im Einklang mit den Richtungen der spontanen Augenbewegungen, die von links nach rechts, von oben nach unten leichter seien und leichter divergieren als konvergieren sollten. Dementsprechend sei es leichter einem Objekt, das sich in diesen Richtungen bewegt, zu folgen, weshalb eine Bewegung in entgegengesetzter Richtung überschätzt wird. Diese Überschätzung wäre dann eine Folge des Umstandes, dass hierzu eine grössere Muskelenergie benötigt würde, womit das Problem auf eine physiologische Basis reduziert wäre. M.E. ist es aber schwerlich anzunehmen, dass bei der Beobachtung einer Bewegung über ein Feld von 15 cm eine Differenz in Quantum Muskelenergie, für das Folgen einer Bewegung nach links oder nach rechts erforderlich, diesen grossen phänomenalen Geschwindigkeitsunterschied erklären könnte. Nach Corkill und Lythgoe gäbe es keine Differenz in der Geschwindigkeit der Augenbewegungen nach links und nach rechts. Takala meint aber, auf Grund seiner Versuche, eine geringe Andeutung erhalten zu haben, dass die Augenbewegungen nach rechts leichter und schneller seien. Bei der Asymmetrie, die in der Organisation der Wahrnehmung von Objekten in Ruhe auftritt, sollten die Augenbewegungen keine Rolle spielen.

Von Takala werden für die Wahrnehmung von Bewegungen also gleichfalls Vorzugsrichtungen angenommen, welche von links nach rechts, von oben nach unten und sagittal in entfernender Richtung laufen müssten. In diesen Richtungen wird die Geschwindigkeit eines Objektes unterschätzt. In seinen Versuchen über die Beurteilung der Geschwindigkeit erlangt Takala aber andere Resultate als von mir gefunden werden. Nun besteht in der Versuchsanordnung eine bedeutende Differenz, die den Unterschied in Ergebnissen möglicherweise bedingt hat. Meine Versuche wurden unter möglichst reduzierten Verhältnissen vorgenommen. In Takalas Versuchen dagegen waren die bewegenden Objekte Quadrate von  $10 \times 10$  cm, die sich im Hellen mit einer Geschwindigkeit von 8 cm/sec. gegen einen schwarzen Hintergrund über einen Abstand von 1.20 m bewegten. Der Beobachtungsabstand war 6 m. Die Anzahl Vpn. war 10; von jeder wurde 6 Einstellungen erhalten. Takala bemerkt hierzu aber selber, dass infolge der mangelhaften Apparatur die Ergebnisse nicht als endgültig zu betrachten seien, ausgenommen den in der sagittalen Dimension gefundenen, wo die Differenzen bedeutend sind. Seine Ergebnisse sind:

- a. Es liegt kein Unterschied in phänomenaler Geschwindigkeit vor zwischen einer Bewegung nach rechts und nach links.
- b. Die Geschwindigkeit der aufsteigenden Bewegung gegenüber der

deszendenten wird überschätzt. Der Unterschied sei statistisch auf dem Niveau der 1 %-Verlässlichkeit gesichert ( $t = 3.3$ ).

c. In der sagittalen Dimension wird die sich nähernde Bewegung gegenüber der sich entfernenden überschätzt. Der Unterschied sei gesichert auf dem Niveau der 1 %-Verlässlichkeit ( $t = 4.3$ ).

Anlässlich *a* kann man bemerken, dass dieses Resultat nicht völlig seiner Erwartung, gegründet auf die von ihm gefundene Asymmetrie in der Organisation unserer Wahrnehmung von statischen Objekten, entspricht. Zwar wäre, nach Takala, die Asymmetrie der horizontalen, im Gegensatz zu der der vertikalen Dimension, unter weniger reduzierten Verhältnissen geringer. Da seine Versuche im Hellen und mit einer Expositionszeit von 15" vorgenommen wurden, die meinigen dagegen im Dunkeln mit einer Expositionszeit von 1.5", ist es sehr gut möglich, dass hierdurch der Unterschied in den Ergebnissen verursacht worden ist. Der Einfluss der Erfahrung wird wahrscheinlich eben in der horizontalen Dimension wohl kompensierend auf die ursprüngliche <sup>8</sup> Asymmetrie wirken.

*Ad b.* Die steigende Bewegung wird überschätzt, was der Asymmetrie entspricht, die er in der vertikalen Dimension in den Organisationsphänomenen unserer Wahrnehmung von Objekten in Ruhe findet. Takala erwähnt, dass der Unterschied auf dem Niveau der 1 %-Verlässlichkeit gesichert sei. Dies ist aber nicht richtig: die Sicherheitsschwelle ist nur 5 %. Da zudem die Apparatur als wenig verlässlich von ihm betrachtet wurde, ist dieser Unterschied wohl einigermaßen fragwürdig.

Takala teilt nachstehende Daten mit:

Der Standard bewog sich nach unten, die Variable nach oben. Die Zeitdifferenz war, über einen Abstand von 1.40 m gemessen, durchschnittlich  $V_1 - S = 1.8''$ . Daraufhin wurde die Reihenfolge der Bewegungen umgekehrt und war der Zeitunterschied  $V_2 - S = -0.2''$ . Er nimmt nun die Differenz zwischen  $V_1$  und  $V_2$ , die 2" beträgt und basiert hierauf die *t*-Berechnung. Da der Effekt der Täuschung in  $V_1$  und  $V_2$  entgegengesetzt ist, macht 2" also den doppelten Wert des tatsächlichen Unterschieds zwischen einer steigenden und einer fallenden Bewegung aus. Nun besteht kein Bedenken dagegen, diesen doppelten Wert in Anrechnung zu bringen, vorausgesetzt dass man dann darauf achtet, dass es wahrscheinlich eine grosse negative Korrelation zwischen den beiden Methoden gibt. Der theoretische Wert des Korrelationskoeffizienten beträgt  $-1$ , und man darf annehmen, dass der empirische Wert gross ist, umso mehr da auch die Streuungen in beiden Fällen gleich sind, nämlich 1.2". Takala berücksichtigt die Korrelation jedoch nicht. Als durchschnitt-

<sup>8</sup> Wenn ich von einer ursprünglichen Richtungstendenz rede, ist hiermit nicht gemeint, dass diese für Kinder ebenfalls oder vielleicht stärker gelte. Es ist sehr gut möglich, dass dies nicht der Fall ist. Dies bedarf aber einer näheren Untersuchung.

lichen Unsicherheitswert über beide Methoden erwähnt er  $0.6''$  und berechnet  $t$  aus  $2'' : 0.6'' = 3.3$ . Da er nur die Durchschnittsdifferenz der 10 Vpn. erwähnt, ist der Korrelationskoeffizient nicht zu berechnen, aber  $t$  wird bestimmt kleiner sein als 3.3. Der theoretische Wert ist  $t = 2'' : 0.8'' = 2.5$ . Da der kritische Bruch bei 9 Freiheitsgraden auf dem Niveau der 1 %- bzw. 5 %-Verlässlichkeit resp. 3.25 und 2.26 beträgt, ist der Unterschied also nur auf dem Niveau der 5 %-Verlässlichkeit gesichert.

Da die Streuungen für jede Methode  $1.2''$  betragen und die Grösse der Täuschung durchschnittlich  $1''$  ist, gilt die Täuschung auch nicht für alle Vpn., sondern entweder nur etwa für die Hälfte, oder zwar für die Mehrheit, aber dann gibt es sogar einige Vpn. mit entgegengesetzter Täuschung. Da Takala die individuellen Werte nicht erwähnt, ist dies nicht festzustellen.

*Ad. c.* Hierfür gilt mutatis mutandis dieselbe Auseinandersetzung. Der theoretisch zu erwartende Wert von  $t$  ist 3.09, so dass es fragwürdig ist, ob der Unterschied auf dem Niveau der 1 %-Verlässlichkeit gesichert ist.

Dass in der sagittalen Dimension die sich nähernde Bewegung phänomenal überschätzt wird, können wir als im Einklang mit unserer Hypothese betrachten.

Vergleichen wir meine Resultate mit denjenigen Takalas, so möchte ich zur folgenden *Konklusion* kommen.

Wir besitzen im Raum Vorzugsrichtungen, in denen wir uns orientieren. Diese verlaufen horizontal von links nach rechts, vertikal von oben nach unten und sagittal in entfernender Richtung. Da wir existentiell auf die Begegnung mit den Dingen gerichtet sind, die mit uns unsere Welt konstituieren, gibt es in der Wahrnehmung Organisationstendenzen, die die Gegenstände auf uns hin gerichtet strukturieren, so dass die Vorzugsrichtungen, in denen statische Objekte wahrgenommen werden, nach links, nach oben und sagittal auf uns hin gerichtet sind.

Da wir uns als expansive Wesen in der horizontalen Ebene unterwegs befinden, unterschätzen wir die Geschwindigkeit eines Objektes, das sich in derselben Richtung wie unsere Orientierungsrichtung bewegt und überschätzen wir die Geschwindigkeit in entgegengesetzter Richtung. In der vertikalen Dimension befinden wir uns nicht unterwegs, sondern haben hier gleichsam den Endpunkt als Ruhepunkt erreicht, was unserem Dasein seine Stabilität garantiert. Daher findet keine Überschätzung der steigenden Bewegung statt.

## DO MICRO-GENETIC SEQUENCES REFLECT LIFE HISTORY? AN ILLUSTRATION

BY

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The concept of micro-genesis (*Actualgenese*) was introduced by the Leipzig psychologists. Every piece of behavior was regarded by them as a condensed development, a series of events. One typical experimental procedure may illustrate how they managed to analyse this micro-genetic process in, e.g., an act of perception. The stimulus, most often visual but sometimes auditory, was presented, at first blurred, about the *minimum perceptible*, then, gradually all the more clear until it became thoroughly recognizable. By thus gradually facilitating the perceptual treatment the experimenters could see how the "perceptual process" passed through different developmental stages until it reached its relatively stable level, "ordinary" perception.

The following comment by Felix Krueger might suffice here to summarize one of the most important aspects of the Leipzig research: "Besides establishing results for the genetic primacy and for the penetrating, phenomenological, and functional dominance of the feelings, he (i.e. Sander) gained instructive new views, even of the structural condition of those processes. In that he regularly cut back the effect of the outside stimulus . . . he showed . . . that the dispositional sets became preponderant; in this manner, certain mental part-structures and their persistent cohesion became clearly recognized" (8, p. 81, cf. 12). Metzger (10) related the concept of micro-genesis more explicitly to that of ontogenesis.

Such a formal correspondence between stages in a micro-genetic process and stages in ontogeny has later been particularly emphasized by American students (16, cf. 1, 9, 13). Framo, using Rorschach cards in a tachistoscopic

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<sup>1</sup> The present authors want to express their gratitude to Mr. J. Flensburg, who introduced the phototest, performed a good deal of the testing, directed the first scoring of data, and, later, followed the development of this paper with unflinching interest. Among many friends suggesting changes in the manuscript mention should be made especially of Mr. Jan Bruell, Clark University, and Dr. Daniel J. Levinson, Harvard University.

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study of perceptual development in normal adults, comments upon his results as follows: "The development of the perceptual process in normal adults appears to recapitulate the perceptual development of the maturing child" (6). The present paper also deals with the problem whether a sequence of reactions to stimuli presented tachistoscopically reflects life development. In contrast to the Leipzig school, however, our approach is explicitly personality oriented. Furthermore, the use of physiognomic material for a genetic analysis of interpersonal relations marks a difference with respect to the American studies as well.

The basic problem in this tachistoscopic experiment concerns the correspondence between response to photographs and relations with the parents, or, to be more specific, between temporal stages in "phototest" response and the genesis of experience with the parental family. In order to carry out such an analysis we had to describe our comparison groups in clinical-dynamic dimensions. Originally derived from an attitude test regarding the parents, however, these groups showed considerable heterogeneity as far as life development was concerned. Although it was possible to demonstrate repeatedly that differences between two comparison groups in their phototest sequences corresponded to differences in life history our results were perhaps more suggestive than conclusive. The aim of this paper is, therefore, to present the phototest and to illustrate its possibilities for future research.

#### THE PHOTOTEST

The phototest consisted of 30 pictures, 3 pictures of each of the following groups: older men, older women, middleaged men, middleaged women, young men, young women, adolescent boys, adolescent girls, small boys, and small girls. The selection of pictures was based on an earlier investigation of employees and university students and was suited to the new groups of Ss, students preparing for social work and students at the university. All of the pictured people were Swedish and the pictures recently taken; most of them, like the Ss, represented the middle class. They generally exhibited only face and shoulders. None of them was known to our Ss.

The tachistoscope, constructed by Flensburg, has two projectors with compure-shutters and a timer device, allowing exposures from 1/200 of a second upwards; colored gratings, used to extinguish positive after-images, can be exposed immediately afterwards or with a pause between the picture and the grating. The pictures appeared on a milky glass plate, their size being about 30 cm on a side. Investigations by Ehrenstein (4) show that, within certain limits, a prolonged time distance between picture and



grating is equivalent to a prolonged exposure time. Therefore, we included, in the total exposition, both exposure time and time distance between picture and grating (see Table I).

TABLE I  
Exposure Times

Presentation *	Sequence of exposures				
	1st	2nd	3rd	4th	5th
Exposure of stimulus . . . . .	.01	.04	.10	.20	30
Interval between exposure and colored grating . . . . .	.04	.04	.04	.07	—
Total exposition . . . . .	.05	.08	.14	.27	30

\* Time in seconds.

We started with short exposures, gradually prolonging them in order to follow a sequence of answers. In the 5th exposure the pictures were presented at 30 seconds without a subsequent grating. The first four exposures were originally fractioned in a Weber scale, as has been recommended by Wohlfahrt (17). For practical reasons we modified our scale later, however; preliminary experiments convinced us we should raise the 1st exposure in order to get answers. The meaning of the sequence will be viewed more in detail below. An outline of the theoretical considerations underlying the development approach can be found in earlier papers (14, 15).

Ss were instructed to report (as soon as they perceived a picture) the sex of the pictured person, their degree of certainty as to sex, and their degree of sympathy. The dimension of sympathy, the only one considered here, was scaled on five degrees: very sympathetic (+2), sympathetic (+1), neither sympathetic nor unsympathetic (0), unsympathetic (—1), very unsympathetic (—2). All of the pictures were first presented at the 1st exposure time, then all at the 2nd, and so forth. Thus, we limited the inclination to build stable hypotheses on any single picture (3, 11).

Here we are primarily concerned with the degree of sympathy. The meaning of the intervals is unknown, however, and the distribution of reactions is sometimes rather skewed. Therefore, it was advisable to use T-scores. The results presented thus do not denote absolute values but a deviation from the group average with sigma used as a unit.



## BASIC ASSUMPTIONS FOR THE PHOTOTEST

We base our choice of comparisons on the (common) assumption that reactions to older and middleaged men represent experience with the father, reactions to older and middleaged women experience with the mother, etc. Regarding the method of prolonging exposure times, we want to imply that reactions to pictures at short times, being difficult to perceive thoroughly, will very likely be more basic or "direct" than reactions at long times, thus representing less controlled, less isolated, or, more "personal" forms of behavior (6, 16). These more "unguarded" reactions may have more direct relations to early stages in ontogeny than have the more prepared and defended ones.

Sympathetic and unsympathetic answers are interpreted according to Szondi. Negative reactions then symbolize that needs of contact have been repressed, or, that they, owing to some outer obstacle, could not be satisfied. We may, in the widest meaning of the word, talk about broken contact or negative cathexis (5). A positive answer points at a latent need which is not satisfied at the moment but the satisfaction of which is accepted (positive cathexis). The more neutral reaction would imply that the need is being satisfied at the moment; there is no longer any urgent tension in the person. With the sigma scoring, however, the neutral reaction does not necessarily mean the alternative "neither sympathetic nor unsympathetic" but equals the average reaction of the sample to a special group of pictures. Hence, we will study group *differences* rather than levels.

## CHOICE OF COMPARISON GROUPS

Altogether 63 Ss took part in the experiment, 37 men and 26 women. In this illustration we will use two extreme female groups of 8 Ss each. They were chosen with respect to the number of times the statement "Children without a father are as happy as children with one" was preferred in pair comparisons with 14 other statements on related matters. The *Low-group* rejects the statement in most of the comparisons, i.e., they acknowledge the positive role of the father; the *High-group* accepts the statement—denying the father. Since we are concerned with a statement regarding one of the parents we will use only reactions to pictures of older and middleaged people for our comparisons.

The clinical description of these groups was based on the Szondi and TAT tests and on interviews and questionnaires. We are here concerned with group profiles of Szondi results and take into account only significant *differences* between the groups. The statistical analysis was applied not

only to single factors, single need-tensions underlying the preference or rejection of, e.g., pictures of solely epileptics and paranoids, but also to such combinations of factors which, according to Szondi, may represent syndromes.<sup>2</sup>

The TAT battery included 8 pictures, three of them being composed in Lund and the rest belonging to the standard series. One of the new pictures (I) shows a boy looking through a broken window; the second one (II) a man and a woman, the latter sitting at a table and the former standing in front of her; the third picture (III) is a family scene, the father turning his back to the camera, the mother seen in profile, and a boy in full view. The sequence of pictures was, for female Ss: 14, III, 7GF, 3BM, 6GF, II, 13MF, I.

The main interpretation principle was to find a unity theme (as Murray has recommended), which was at the same time as far as possible developmental, i.e., differentiated into primary and secondary stages. The interviews, also containing material from an extensive questionnaire of about 700 items, were used to complete the TAT interpretations and to verify them. The TAT interpreter (Kragh) did not know the phototest results beforehand—not until he had given a definite description of the comparison groups.

## RESULTS

The two female groups chosen with respect to the statement "Children without a father are as happy as children with one" seem to be clinically clearly different. According to Szondi, the *Low-group* tends to accept more of active-masculine sexuality than does the *High-group* (more s+;  $P < .02$ ), and it is also more paroxysmal (more hy+;  $P < .05$ ). Its representatives are thus more dynamic, active, and effective, more suggestive and masculine. The TAT stories and the casuistic material seem to confirm this sketch of the *Low-group*. We observe introjection of a male role in most Ss, who are positive toward men except for a negative period before the introjection. Some few Ss do not give full evidence of such an introjection but display open aggressiveness toward women. Thus, the attitude seems to express full acceptance of the father as an object of identification.

The life histories of the *High-group*, as unveiled in the TAT, the questionnaires, and the interviews, are very similar in all Ss. From the beginning they have been frustrated by one, or, most often, by both of the

<sup>2</sup> Discussions of the validity of the Szondi test can be found in two recent papers (2, 7).

parents. Thereafter, they try to introject one of them but do not succeed. In the end we find loneliness, depression, and narcissism, the consequence of an unsuccessful introjection or identification. These life histories are obviously reflected in and confirmed by the Szondi test, describing the High-group as more passive-feminine (see above), and also as more conscious, more inflative and introjective (more (k+) + (p+);  $P < .05$ ). The attitude might be an attempt to rationalize the loss of the father; they are at least aware of the fact that they have lost him.

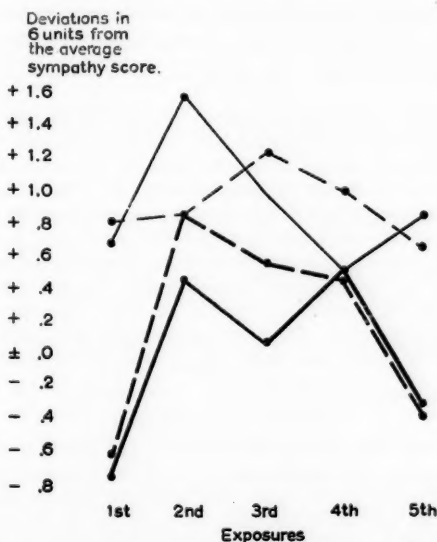


Fig. 1. Reactions in two female extreme groups to photographs presented tachistoscopically

As shown in Fig. 1, the Low-group is generally more positive in the phototest than is the High-group. The differences are significant at the 1st ( $P = .01$ ) and 5th ( $P < .05$ ) exposures. The Low-group grows more positive from the 1st to the 2nd exposure, especially in its reactions toward men; and, in spite of a general lowering of level, the final reaction is still positive. The High-group has a similar development to begin with, though all the way more negative, remains on a rather even level up to the 4th exposure, but once again becomes negative at the 5th exposure.

The sequence of reactions toward male pictures in the Low-group,

rising from the 1st to the 2nd exposition, and remaining positive at the last exposition, seems to coincide with the findings that, through introjection, the Low-Ss get better contact with the father and that the introjection finally succeeds. The High-group, early frustrated by both of the parents, is more negative than the Low-group to begin with. Later, there are obvious attempts at introjection, in the phototest probably reflected by the increase of positive response from the 1st to the 2nd exposure; but we learned that the introjection was a failure and get signs of broken contact at the 5th exposure. The difference in this part of the phototest should, according to previous assumptions, symbolize the actual difference between the groups; and it expresses the positive cathexis in the Low-group as compared with the negative cathexis in the High-group, i.e., the attitude *vis à vis* the father.

It may be worth mentioning, in addition, that the female Low-group described above differed also from an equivalent male Low-group (10 Ss).<sup>3</sup> This later group neither succeeded in getting contact with the father by the way of passive (inflation) identification nor active (simple introjection).<sup>4</sup> They were likewise deeply frustrated by the mother. In some female Ss we found aggressiveness toward the mother, but, in their introjection of a male role, they were able nevertheless to get contact with her (and with the father). The male Low-group was generally more orally frustrated, the female group more organizationally (anally); there was thus a deeper frustration in the men than in the women. Consequently, the male Low-group was much more negative toward both male and female photographs ( $P = .001$ ), the difference becoming especially obvious when after the 1st exposure the female Ss grew more positive.

#### SUMMARY AND DISCUSSION

Sequences of sympathy reactions to photographs of older and middleaged men and women presented tachistoscopically at gradually prolonged exposures were compared in two female groups chosen with respect to rejection or acceptance of a statement regarding the role of the father in family life. A clinical description of the two groups was provided by projective tests, interviews, and questionnaires. The illustration presented here was part of a more extensive investigation including a variety of comparison groups (e.g., the presentation in small print above) and photographs of people of all ages, an investigation carried out in order to demonstrate that the history of interpersonal relations might be reflected in phototest sequences. Since many of the comparison groups tended to be clinically very heterogeneous the results could not be fully conclusive.

<sup>3</sup> The TAT pictures used for the male group were: 14, III, 8BM, 3BM, 6BM, II, 13MF, I.

<sup>4</sup> Passive identification should, according to Szondi, be distinguished from active, the latter being about the same as identification in, e.g., Fenichel (5). The passive (inflation) identification is related to oral-receptive contact, i.e., going up into, serving, joining the object, as contrasted to assimilating, introjecting. The concept has proved to be empirically useful.

Insofar as the comparison groups were consistent with respect to individual life histories, however, statistically significant and intelligible differences were obtained in the phototest.<sup>5</sup> Most of these differences confirmed the assumption that male pictures represented the father and female pictures the mother, and that negative and positive reactions could be interpreted according to Szondi. It is even more important to note that serial characteristics in the phototest have often corresponded to the basic theme of life history within a group, as, e.g., that an early frustration by the mother followed by successful attempts at identification is reflected in primary negative reactions to older and middleaged women which grow more positive as the exposure times are gradually prolonged.

Although we have not been able conclusively to prove our primary assumptions concerning the phototest, we believe that we have at least suggested a new field of application for the tachistoscopic technique. It seems plausible to venture that, by means of this technique, it is possible to test, for instance, whether primary experiences have been transformed and overbuilt or have kept their quality and object even at a more conscious level, i.e., to test the vertical structure of a behavior complex, a structure which often coincides with the phases of a life history.

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# DENKEN UND SPRECHEN IN IHRER GEGENSEITIGEN ABHÄNGIGKEIT

VON

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Meiner Meinung zufolge, wurden in der Diskussionsnummer der Acta Psychologica zum Thema „Denken und Sprechen“ gerade die grundsätzlichen Distinktionen nicht oder nicht mit genügender Schärfe berücksichtigt. Diese Zeilen sollen dazu bestimmt sein, die nötigen Distinktionen aufzuzeigen.

Es muss grundsätzlich unterschieden werden einerseits zwischen Denken und Sprechen als soziale Betätigungen, als Erscheinungen, welche durch das gesellschaftliche Zusammenleben in der gemeinschaftlichen Arbeit geschaffen wurden und durch das gesellschaftliche Zusammenleben aufrechterhalten werden, andererseits zwischen Denken und Sprechen als physiologische Vorgänge, durch deren Ingangsetzung die erstgenannten sozialen Erscheinungen auf die Bühne treten können.

(1) Als *soziale Phänomene* müssen Denken und Sprechen eng aneinander gebunden sein. Es wäre Unsinn zu erwarten, dass Sprechen ohne Denken, Denken ohne Sprechen soziale Phänomene bilden könnten. Sogar das Bildwerk eines Malers, wenn es etwas der Gesellschaft zu sagen trachtet und das will es eben, besitzt einen kurzen, in Worte gefassten, Hinweis, was es verkünde. Diesem sozialen Standpunkt wird die Pawlowsche Annahme eines Signalsystems zweiter Ordnung gerecht, indem dieses System Sprache und Denken beides gleichsam umfasst.

(2) Als *physiologische Funktionen* scheinen Denken und Sprechen ebenfalls dicht aneinander zu wirken. Dafür spräche auch der obige Hinweis auf Pawlow's Signalsystem zweiter Ordnung.

Doch hier benötigen wir eine zweite Klarstellung: *Was bedeutet denn solch ein Dicht-aneinander-Funktionieren in den physiologischen Abläufen?* Ohne diese Klarstellung sind die gesammelten Beispiele bezüglich Denken und Sprechen ohne Wert. Man muss eine Umschau unter anderen physiologischen Abläufen halten um auf diesen speziellen Gebiet klar zu sehen.

Das Angewiesensein einer Funktion an einer anderen findet man sozusagen als Regel in der Physiologie. Kann das Herz ohne Lunge, kann die

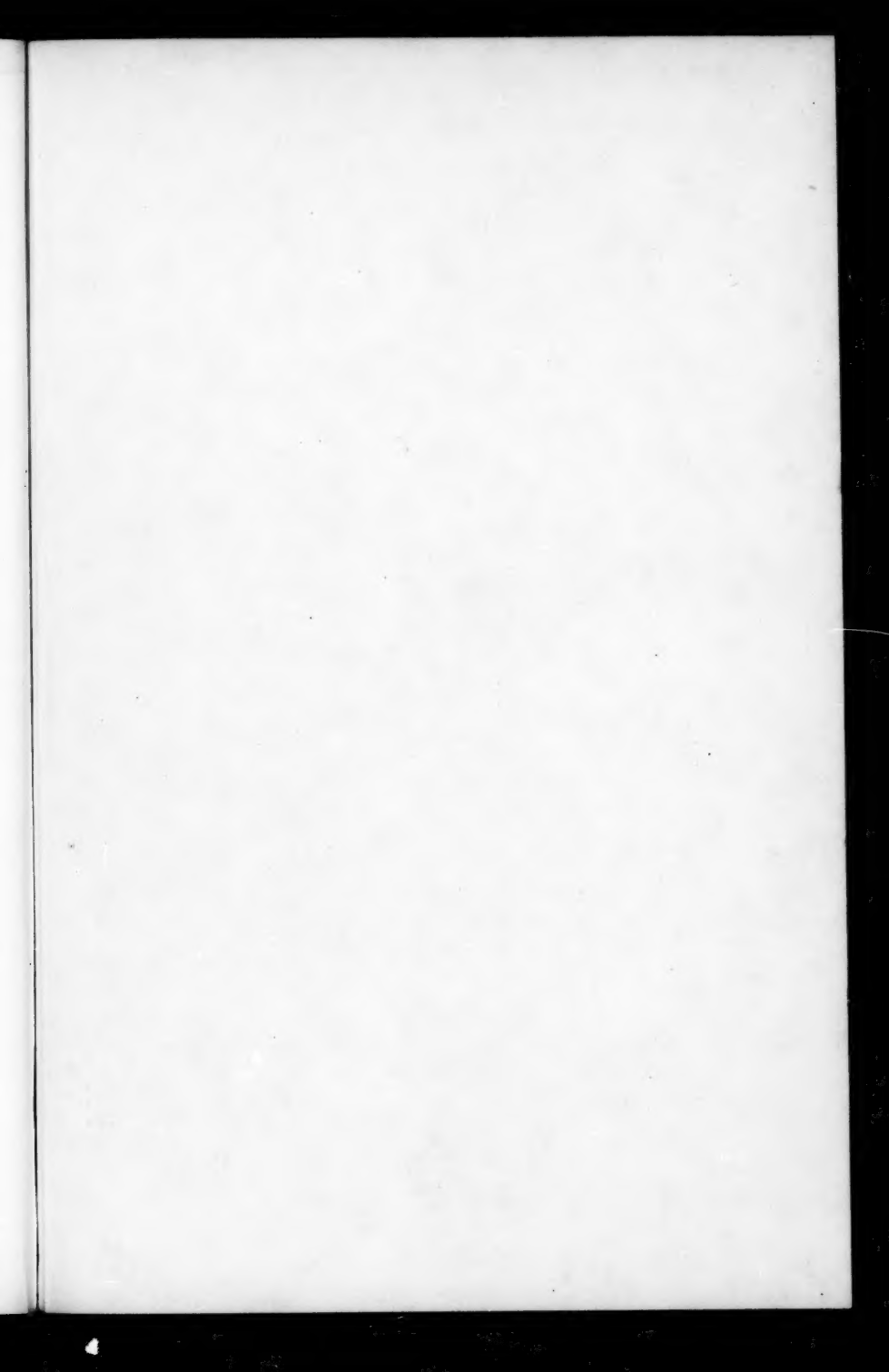
Lunge ohne Herz arbeiten? Die erste Antwort wird sicher lauten: sie können nicht. Kann überhaupt ein Körperteil ohne Gehirn seine Aufgabe verrichten? Und kann das Gehirn ohne Herz, Lunge, Niere usw. sich betätigen? Die erste Antwort lautet wiederum: nein, natürlich können sie das nicht vollbringen. Raum kann nur mit Hilfe anderer Empfindungen, anderer Wahrnehmungen empfunden, wahrgenommen werden. Diese Empfindungen, Wahrnehmungen sind sodann entweder ihrerseits mit „Lokalzeichen“ behaftet, oder — sie können nicht existieren.

Und dennoch: Im embryonalen Leben arbeitet das Herz ohne Lunge (ohne eigene Lunge); Gewebe können ohne Gehirn künstlich gezüchtet werden, das isolierte Herz kann künstlich in Funktion gehalten werden. Und das zweite Signalsystem kann dafür bürgen, dass, wenn auch kein lebendiges Raumerlebnis, so doch wenigstens nachfühlbare Gesetzmässigkeiten nur theoretisch konstruierter, nicht wahrnehmbarer Räume bestimmt und anderen mitgeteilt werden können. Das heisst aber, dass *unter Umständen, in der Natur vorgefunden oder künstlich hergestellt, aneinander streng angewiesene Funktionen auch isoliert, einzeln arbeiten können.*

Nun findet sich *in der Traumarbeit* und ihr ähnlichen Leistungen eine Denkarbeit ohne Sprache, ohne Worthilfen. (N.b.: Die Sprache *im* Traume gehört erfahrungsgemäss nicht zur eigentlichen Traum-Denkarbeit, sondern zum Material des Traumes.) Der Traum muss aber zu den physiologischen Abläufen, *ohne* einem sozialen Wert, zugeteilt werden. Das, was Goldstein als „Selbstverwirklichung“ anschauen will, (*Acta Psychol.*, 1954, 10, 176) wirkt sich eben im *asozialen Traum* aus, nicht aber in den sozialen Verzweigungen des Denkens und der Sprache.

Widerspricht jedoch diese Auffassung eines isolierten (gegenüber der Sprache isolierten) Denkablaufes nicht den physiologischen Voraussetzungen gerade der erwähnten Pawlow'schen Annahme? Nein, sie widerspricht nicht, denn erstens sind in der Pawlow'schen Gehirn-physiologie alle Analysatoren mit Keimen einer Denktätigkeit, mit der Fähigkeit zur Analyse und Synthese ausgestattet, zweitens können die dynamischen Verhältnisse im zweiten Signalsystem sowie diejenigen zwischen erstem und zweitem Signalsystem so verändert werden, dass die dynamischen Stereotypen *der Sprache gehemmt* werden diejenigen *der Denkarbeit* jedoch unter Einmischung des ersten Signalsystems, so wie es in den traumnahen sog. phasischen Zuständen zu beobachten ist, *stärker hervortreten.*





The first of these is the fact that the weather was very warm and sunny. The second is that the water was very clear and the fish were very numerous. The third is that the fish were very large and the fourth is that the fish were very healthy.

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